Essays in Applied Labor Economics

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Preface

I am indebted to many people for their help and support in the process of completing this thesis. First and foremost, I want to thank my advisor, Josef Zweimüller. I started working for him as a research assistant during my master studies in economics following a job advertisement on the website. Initially, I was hired to take care of the huge amount of data at the chair, which remained part of my duties until the very end. During the job interview with Josef, we talked about why I am interested in economics and empirical work and what I can expect from the job and the environment at the chair. Luckily, I was hired and started working for and with Josef. What I did not know at that time is that I would learn a lot from Josef and that he would gratefully offer me to stay with him and to start my doctoral studies. Josef is genuinely creative and provides an inspiring work environment. I benefited enormously from his support and advice throughout the years. He helped me to stay focused on the important aspects of my research projects. I am also very grateful to Rainer Winkelmann for agreeing to be the co-advisor of this thesis.

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Contents

1 Introduction

My thesis sheds light on three empirical research questions related to the intersection of the fields of applied labor economics and empirical public economics: What is the long-run effect of more generous unemployment insurance on post-unemployment outcomes? How do unemployed job seekers search for jobs when both wage and commuting distance matter? How do disability insurance recipients adjust labor supply in response to financial incentives? Chapter 2 shows that more generous unemployment insurance has a negative impact on post-unemployment earnings in the long-run; although earnings differences become smaller over time, a gap in earnings remains. In particular, this effect seems to be driven by older job seekers who rely on unemployment assistance for an extended period of time. Chapter 3 shows that both wage and commute distance are relevant for job search, namely, laid-off workers start looking for new jobs in the old workplace that pay a relatively high wage. As time goes on, however, workers increasingly prospect areas outside of the previous workplace and eventually accept jobs that eventually pay a lower wage. Chapter 4 shows that the labor supply of disability insurance recipients is lower than it could be, because recipients reduce their earnings due to financial incentives that cut disability benefits if their earnings are too high.

All three chapters make use of two key ingredients to answer these desired research questions: rich data and an adequate empirical strategy. All chapters are based on the Austrian Social Security Database (ASSD), which covers the universe of private sector wage earners from 1972 until today. It collects complete labor market histories along with socio-economic characteristics and employer information. The data can be linked to several other datasets to gain additional information. I also link the ASSD to the unemployment register to extract further socio-economic characteristics and information about type and amount of unemployment benefits (see Chapters 2 and 3). The link to the tax register allows me to gain information about the place of residence for each individual (Chapter 3) and higher quality information on labor earnings, a crucial ingredient for Chapter 4. The empirical strategies I use vary according to the chapter. For example, in Chapter 2, I compare job seekers before and after the reform by adopting a quasi-experimental, Differences-in-Differences, approach. For Chapter 3, we pair a calibration exercise with Cox-proportional hazard estimates that rely on kinks in the unemployment benefit schedule (Card et al., 2015) and discontinuities in eligibility rules for potential unemployment duration. For Chapter 4 we implement a bunching estimator (Kleven and Waseem, 2013) to estimate the counterfactual earnings densities of disability insurance recipients. The remainder of this introduction presents extended summaries of these three chapters.

Chapter 2: Unemployment Insurance and Post-Unemployment Outcomes

Unemployment insurance systems are present in many countries and serve to replace income in case of job loss. A widely debated issue is what the optimal level and duration of unemployment benefits should be. Increasing the potential duration of unemployment benefits (PBD) gives job seekers more time to find suitable jobs when they need it. There is abundant evidence showing that more generous unemployment insurance prolongs effective unemployment and nonemployment duration because job seekers eventually lower their search efforts in anticipation of the longer benefit duration. Prolonging job search can lead to worse outcomes for the unemployed if employers discriminate against those with longer unemployment, for example, by giving them worse job offers. Thus, giving job seekers more time may not necessarily help them to find better jobs. In this essay, I answer the question of what happens to earnings after unemployment for those unemployed who are allowed to spend more time on unemployment benefits. I use a reform in Austria that increased PBD for job seekers above age 40 with sufficiently high experience, while it remained unchanged for less experienced or younger job seekers. The reform is analyzed utilizing a Differences-in-Differences strategy contrasting the inflow of treated and control groups before and after the reform.

While there is no discernible earnings difference between treated and controls before the reform, earnings are substantially lower in the first year after unemployment entry for job seekers eligible for the extension in PBD. Earnings are lower because treated are nonemployed longer and also because they find worse jobs as measured with daily earnings and employment days. Earnings remain low for a substantial amount of time, but the difference in earnings shrinks as the treated eventually leave their current employers and find a better match. Extending PBD has a differential impact by age group. Specifically, young job seekers incur a relatively smaller earnings loss and virtually everyone returns to employment. The difference in earnings declines quickly, but the treated do not overtake controls implying a gap in earnings over the entire 10 years that I assessed. Old job seekers incur a larger earnings loss, remain nonemployed longer, and rely more heavily on unemployment assistance as a consequence of the reform. Overall, the reform did not help either of the groups to find better jobs or find jobs fast enough to avoid detrimental effects from prolonged joblessness.

Chapter 3: Spatial Search Strategies of Job Seekers and the Role of Unemployment Insurance

Most people do not work where they live and have substantial commute times to work. On average in OECD countries, individuals travel 60 minutes one-way. Standard models of job search focus on wages as main decision variable and do not account for the role of space, which might be a useful simplification. We consider the distance-wage trade-off by assessing the role of space empirically but also by extending the standard job search framework.

We use the ASSD combined with the unemployment register and information on travel times between municipalities within Austria to study the interplay of distance and wage empirically. An unemployed individual can leave unemployment in six distinct exits: higher/lower/same wage and higher/lower/same distance. Using the different exits as competing risks, we pursue a Cox regression approach to estimate hazard rates for each exit as a function of unemployment parameters such as benefit level and benefit duration. The econometric specification uses three features of unemployment insurance in Austria. First, we make use of the fact that the level of unemployment benefits is a kinked function of previous earnings. Second, unemployment benefit duration is a discontinuous function of age. Third, we explicitly model the finite nature of unemployment benefits and the fact that individuals may enter a second tier regime, unemployment assistance. Empirical results suggest a negative relationship between unemployment benefit level and potential benefit duration and hazard rates. Furthermore, we document a decrease of the hazard rate in the same city relative to the hazard rate outside the city and that over time job seeker accept lower wages.

Then, we develop a simple continuous-time search model to rationalize the empirical observations. In the model, we assume two types of job seekers: covered workers are entitled to unemployment benefits and uncovered workers receive lower unemployment assistance. All job seekers start out in the covered regime but switch to the uncovered regime with a constant rate. Both types target their search to the previous workplace but also outside and we assume that covered job seekers have a relatively higher efficiency of search in the previous workplace. Job seekers make a choice about three things: a minimum acceptable wage (reservation wage), a maximum acceptable distance (reservation distance) and within the reservation distance the optimal intensity of search effort. The solution consists of a reservation frontier, that is, a reservation wage for any commute distance. The slope of the reservation frontier is the marginal rate of substitution between wage and distance: job seekers can buy short commutes with a lower wage or seek to be compensated with a higher wage for long commutes. Uncovered job seekers value unemployment less because of the lower amount of benefits that they receive. Thus, they ask a lower wage for any given commute distance. The fraction of job seekers who finds a job in the previous workplace decreases over time. The main reason is that the average search intensity in the previous workplace declines as more and more uncovered job seekers remain in the pool of unemployed. We simulate the dynamics of the main outcomes of the model over the turn of nonemployment and compare them to the data. The calibrated model is quite able to replicate the empirical regularities. It predicts that the absolute and also all six sub-hazards decline. Furthermore, it is able to replicate the decrease of the hazard rate in the same city relative to the hazard rate outside the city. The analysis shows that both distance and wage matter for job search and that both dimensions can be incorporated into a job search framework.

Chapter 4: Financial Incentives and Earnings of Disability Insurance Recipients: Evidence from a Notch Design

Disability insurance (DI) programs are among the largest social programs by now and amount to approximately 2.5 percent of GDP on average (OECD, 2010b). While DI programs are designed to provide income replacement in case of permanent loss of earnings capacity, many programs have been criticized to discourage work. One work disincentive present in many countries is that beneficiaries lose part of their benefits if earnings exceed a substantial gainful activity amount (SGA) amount.

For Austria, we find that the presence of this SGA cap induces many DI beneficiaries to adjust their monthly earnings just below the SGA threshold of €439. If monthly earnings exceed the SGA threshold by €1, DI benefits are reduced by up to 50 percent in that month. These rules generate a discontinuous increase in the (implicit) tax liability – a notch – at the SGA threshold and create a strong incentive to bunch below the SGA threshold. Estimating a counterfactual earnings distribution under the scenario where no notch is present, we find large and sharp excess bunching just below the SGA threshold. We estimate that DI beneficiaries who earn just below the SGA threshold would increase monthly earnings by up to €400 if the notch at the SGA threshold did not exist. This represents a 91 percent increase relative to the SGA earnings level. The estimated earnings response is large, but the implied earnings elasticity (i.e., the amount of earnings change as a response to a change in the implicit tax rate) is small with only 0.206. This is because notches create extremely large implicit tax rates, and thus, the behavioral responses are large even when elasticities are quite small. Simulating alternative policies, we find that replacing the notch at the SGA threshold with a €1 benefit offset for every €2 of earnings would increase work and reduce government expenditures.

2 Unemployment Insurance and Post-Unemployment Outcomes

2.1 Introduction

This paper studies how the generosity of unemployment insurance (UI) affects post-unemployment outcomes. Understanding whether a policy change impacts post-unemployment outcomes is crucial to assess the implications of a reform beyond unemployment itself. Evaluating the effect of changes in UI on unemployment itself is too narrow if unemployment insurance also affects job quality. Fiscal cost of increasing UI may be outweighed if job seekers can explore more job opportunities to find better jobs. But fiscal costs are even higher if increasing UI deteriorates post-unemployment outcomes. Therefore, even a pure policy assessment requires information on how changes to UI affect post-unemployment outcomes. Knowledge about whether and how post unemployment outcomes are affected has implications for the formulas for the optimality of unemployment insurance (Chetty, 2008; Schmieder et al., 2012a).

Theoretical predictions are ambiguous whether and how a longer duration of benefits affects post-unemployment outcomes. Standard job search theory predicts that longer potential benefit duration (PBD) makes job seekers more selective. This mechanism leads to a decrease in the unemployment exit rate (Mortensen, 1977; van den Berg, 1990) and may result in better job match quality because job seekers wait for higher productivity jobs (Marimon and Zilibotti, 1999; Acemoglu and Shimer, 2000). Unemployment benefits can influence job match quality negatively if, for example human capital depreciates or employers discriminate against job seekers based on unemployment duration (see Oberholzer-Gee (2008) or Kroft et al. (2013) for experimental evidence). Both mechanisms might be relevant at the same time. The negative influence is more likely to dominate the higher selectivity of job seekers for longer nonemployment durations (Schmieder et al., 2016; Nekoei and Weber, 2015).

There is a vast literature showing that more generous UI leads to longer duration of unemployment benefit receipt (*unemployment duration*) and/or the time between job loss and re-employment (*nonemployment duration*).[1] More recent evidence also shows how consumption changes during unemployment. Kolsrud et al. (2015) document that consumption drops from the beginning of unemployment and further decreases throughout the spell. Changes in earnings are an important determinant for changes in consumption during unemployment because transfers do not fully compensate for the earnings loss. However, their study does not answer whether there are implications for consumption after unemployment benefits have run out. Surprisingly little is known on how changes in UI impact post-unemployment outcomes. Although the literature on post-unemployment outcomes has grown recently, there neither is a consensus on the sign nor the size of potential effects. Most studies that directly evaluate the effect of more generous UI on post-unemployment outcomes focus on wages and tenure on the first job after unemployment. Other frequently assessed outcomes also include whether individuals switch industries and/or occupations.[2] The set of outcomes is potentially large, yet some are only observed on the first job per definition while others would in principle allow measuring long-term effects of changes to UI. Much less is known on the consequences of more generous UI on longer-term outcomes in general.

Schmieder et al. (2012c) study long-term effects of higher PBD in Germany. PBD discontinuously increases at age 42 from 12 to 18 months. Regression discontinuity estimates indicate that increased PBD leads to higher initial nonemployment. While nonemployment remains larger up to five years after the initial unemployment spell, individuals with longer initial spells have a lower subsequent probability to be nonemployed. Changes in nonemployment could imply that days spent on some form of social benefits changes as well. Assessing the long-term consequences of changes to UI for other social programs is important and interesting to study in light of the complementarity/substitution discussion (see Inderbitzin et al. (2016). Degen and Lalive (2015) study a reform in Switzerland that reduced PBD from 24 to 18 months for older job seekers. They focus on earnings up to four years after unemployment entry. Results indicate that a reduction in PBD leads to permanently higher earnings (3.3 percent) and employment (3.3 percentage points).

[1]Studies include the seminal work by Meyer (1990) using US data, Hunt (1995) for Germany and Winter-Ebmer (1998), Lalive and Zweimüller (2004), Lalive et al. (2006). Studies identifying a causal effect by means of a regression discontinuity design include Lalive (2008), Card et al. (2007a), Caliendo et al. (2013), Schmieder et al. (2012a) and Nekoei and Weber (2015).

[2]see Addison and Blackburn (2000a) for a review of earlier contributions. van Ours and Vodopivec (2008) study a decrease in PBD in Slovenia finding small positive effects on re-employment wages. Mas and Johnston (2015) study a decrease in PBD in the US and do not find that re-employment wages change. Studies that assess an increase in PBD include: Caliendo et al. (2013) and Schmieder et al. (2016) for Germany finding negative effects on re-employment wages, Le Barbanchon (2012b) finds no effect on re-employment wages for France, Nekoei and Weber (2015) finding positive effects for Austria. Guglielminetti et al. (2015) study how wages interact with commuting distance. Their evidence suggest that job seekers tend to increase the search radius, centered on the old workplace, as time goes.

The effect is stronger for high skilled individuals likely because skills depreciate faster for them. While four years is a decently long period of time to measure post-unemployment outcomes, it would be interesting to understand whether effects are persistent beyond this period. Furthermore, decomposing the effect into employment and wages would allow to better understand different aspects of job quality.

This paper analyzes the long-term effects of a reform to the Austrian unemployment insurance. The reform increased (i) PBD from 30 to 39 weeks for high experienced workers aged between 40 and 49 and (ii) PBD increased from 30 to 52 weeks for high experienced workers aged 50 or older.[3] The reform, enacted in August 1989, can be used to assess the increase in PBD in a Differences-in-Differences design. The recent literature has a strong focus on identifying effects of unemployment insurance using regression discontinuity designs, for example induced by age thresholds. While these designs allow for a credible identification of a marginal effect of for example extended unemployment insurance on post-unemployment wages, they are bound to be local. Generalising the estimated treatment effect to the population is problematic. Estimating a reform effect at the threshold is attractive but too narrow if also individuals away from the threshold are affected. Identification is more difficult in this case but the potential gains are large because the treatment effect is estimated from a broader population. The reform I study in this paper is ideal for this purpose. I find that an increase in PBD has a negative effect on earnings in the 10 years after unemployment entry. The effect is strong in the beginning but the difference in yearly earnings shrinks over time. Mainly adjustments in (non)employment contribute to both the negative effect in the beginning and the decline of the effect over time. In particular older job seekers rely more heavily on unemployment assistance after unemployment benefits have expired.

This paper adds in at least three aspects to the existing literature on how PBD affects job match quality. First, I focus on earnings instead of re-employment wages as the main outcome. A job seeker may care for the wage earned on the job after unemployment if the job is permanent. But she may equally care about for how many days she can earn this wage in case of unstable jobs. Earnings are a summary measure of employment and wages. Different than wages, earnings do not hinge upon having a job, hence are always defined. Throughout the paper I use labor earnings and neglect other sources for earnings such as social benefits. If not stated differently, I use the term *earnings* and imply labor earnings.

The concept of earnings is formalized in equation (2.1). Specifically, earnings are positive if a worker is employed. In this case she receives daily earnings for the number

[3] The reform also changed the level of benefits for low-income individuals. The effect of the full reform on unemployment duration is analyzed in Lalive et al. (2006).

of employment days. Daily earnings times employment days yields earnings in a given period. Earnings are zero if a worker is not employed in which case employment days and daily earnings are zero. I use the term *conditional earnings* when I only consider the part of a period when the worker is employed. With *employment status* I refer to whether an individual is employed or not in a given period.

$$\text{labor earnings} = \begin{cases} \text{employment days} \times \text{daily earnings} & \text{if employed} \\ 0 & \text{if not employed} \end{cases} \tag{2.1}$$

The specification makes clear that in a given period there are three major margins where differences in earnings can occur: changes in employment status, employment days or different daily earnings. To study long-term outcomes I focus on the 10 years after unemployment entry but also analyze in detail single years to study adjustment mechanisms over time.

Second, the long-term outcome I analyze implies that I study the labor market situation beyond the first job after unemployment. A worker might care for the earnings on the first job. But she might also care for improvements in earnings for example through improvements on the job or by changing jobs (Jovanovic, 1979; Burdett and Mortensen, 1998) if such possibilities exist. To gain insights about the relevance of such channels, I will focus on the evolution of yearly earnings over the 10 years.

The third contribution of this paper concerns the decomposition of earnings in case they are zero. Equation (2.1) makes clear that earnings are zero if a worker is not employed. I decompose nonemployment and study to what degree unsuccessful job seekers rely on other forms of social benefits once unemployment benefits have run out.

This paper is organised as follows. Section 4.2 discusses the institutional background and data. Section 4.3 explains the econometric framework and tests its assumptions. Section 2.4 presents main empirical results for earnings along with robustness checks. I continue with the decomposition of earnings and assess the labor market situation of job seekers beyond earnings by taking into account further social benefits. Section 4.5 concludes.

2.2 Institutional Background and Data

The unemployment insurance system in Austria is characterised by two main parameters: unemployment benefits replace between 40 and 60% of previous earnings for a fixed amount of time (potential benefit duration, PBD). Once unemployment insurance benefits have expired, job seekers may enter the means-tested unemployment assistance (UA) regime. UA is basically granted for an unlimited amount of time but individuals have to re-apply every 26 weeks.

In 1989, the Austrian unemployment insurance system experienced major changes while there were no changes to UA. Before August 1, 1989 the unemployment system in Austria allowed all individuals who worked 52 weeks within the last two years to claim unemployment benefits for a maximum of 20 weeks. All individuals who contributed to unemployment insurance for 156 weeks or more in the last five years were eligible for 30 weeks of unemployment benefits. With August 1, 1989, the Austrian government implemented a series of changes to PBD. PBD became dependent on age and new experience thresholds. Individuals older than age 40 with contributions to the unemployment system of at least 312 weeks within the last 10 years were eligible for 39 weeks of unemployment benefits. PBD increased to 52 weeks for the age group 50 and older if individuals contributed to the unemployment system for at least 468 weeks within the last 15 years.[4]

Table 2.1: Change to PBD and Treatment Assignment

		Prior UI contributions (weeks)			
		<156	156-311	312-467	≥ 468
	<40	20 (-)	30 (C)	30 (C)	30 (C)
Age	40-49	20 (-)	30 (C)	39 (T)	39 (T)
	≥ 50	20 (-)	30 (C)	39 (T)	52 (T)

Notes: Table 2.1 shows the change to PBD in weeks and treatment assignment (C: Control; T: Treatment; -: not considered) based on age and experience at date of unemployment entry. See text for details.

Table 2.1 illustrates how control and treatment groups are constructed. Treatment status is a function of age and experience. Individuals are assigned to the treatment group if they fulfil the experience criterion and fall in the respective age range at unemployment entry. The policy change can be evaluated using a Differences-in-Differences (DiD) strategy by comparing the total inflow before the reform to the total inflow after the reform.

2.2.1 Data and Sample

For the construction of the sample I use the Austrian Social Security Database (ASSD) described in detail in Zweimüller et al. (2009). The ASSD is a matched employer-employee dataset covering the universe of private sector employees in Austria. For each worker, the

[4]The policy change also increased the replacement rate from 41% to about 47% for individuals with monthly earnings between 5000 and 10,000 ATS (363-727 Euros). Lalive et al. (2006) show how the change in PBD and/or the replacement rate affects the duration of unemployment. To isolate the change in PBD I exclude individuals with low previous earnings.

dataset contains daily information on labor market status, including among others employment and unemployment. I consider inflow from August 1986 to July 1996. For each inflow I calculate the unemployment and nonemployment (time from job loss until a new job is found) duration. I refer to these two durations as *initial* unemployment and *initial* nonemployment duration. By date of inflow I infer experience, age and base earnings relevant for the benefit level[5]. These three variables determine the treatment status. From the ASSD I calculate earnings in the ten years before and after initial unemployment. Earnings are deflated to 2000 Euros using the consumer price index. Specifically, I calculate earnings per employer and then aggregate to single years by weighting the employer specific earnings with employer specific employment days. This procedure also yields number of employment days. Similarly, I infer the number of days on unemployment, other benefits (consisting of unemployment assistance, sick leave, disability, old-age) or inactivity. For the last job before initial unemployment I infer earnings, tenure, industry affiliation and geographical location related to the employer. I further include the complete series of earnings, employment and unemployment days in the ten years before initial unemployment as control. I complement the data with information from the unemployment register covering years 1987 to 1998. It contains information in particular about benefit type (UI/UA), daily benefit amount, education, recall status and family situation. The ASSD allows me constructing a local unemployment rate specific to experience, age, industry and region per month to control for group-time specific unemployment patterns.

I make several sample restrictions. To focus the analysis on a homogenous sample. I analyze laid off job seekers; voluntary quits are identified by a 28 days waiting period. Hence, all individuals who enter unemployment directly come from employment. I focus the analysis on men because virtually all men work full time whereas I cannot tell apart whether a women works part time or only part of the year.[6] I keep only individuals below age 55 at date of inflow. This restriction guarantees that labor market attachment is sufficiently high for older individuals when I follow them up to 10 years after initial unemployment. Essentially, I restrict transitions to early retirement and disability and therefore increase the fraction of unemployed workers returning to employment.[7] Similarly, and to reach a more homogenous sample, I exclude individuals below age 35 shrinking the control group somewhat. Finally, I exclude job seekers recalled by the previous employer because

[5]Note that base earnings are not equal to earnings on the last job before unemployment entry. The benefit level is determined based on earnings in year t-1 (t-2) if the individual applies for unemployment insurance in the second (first) half of the year.

[6]Daily earnings available in the data confounds hours worked and the wage rate. Furthermore, the observed employment spells confounds hours and days worked. This limitation is a major problem for women but not for men. Furthermore, the identifying assumption is unlikely to hold for women (see Appendix Figure 2.10).

[7]see Inderbitzin et al. (2016) for the interaction of unemployment with other social benefits.

they have different search incentives.[8]

2.3 Econometric Framework and Identification

I evaluate the policy change using a Differences-in-Differences strategy where I contrast the change in outcomes of the treatment group over time to changes in outcomes of the control group over time. The regression specification is:

$$Y_{it} = \beta_0 + \delta D_i A_t + \beta_1 D_i + \beta_2 A_t + X'_{it}\beta + \varepsilon_{it} \tag{2.2}$$

where Y_{it} stands for post unemployment outcomes, such as earnings. i indicates an individual inflow in period t. D_i is the treatment dummy and equal to 1 if an inflow belongs to the treatment group and 0 otherwise. A_t is a dummy that takes the value 1 if unemployment starts after August 1, 1989 and 0 otherwise. δ is the coefficient of interest that identifies the average treatment effect on the treatment group. X'_{it} is a vector of control characteristics including past earnings, employment and unemployment, education, age, experience, nationality, marital status and indicator variables for industry, geographic location, calendar year and month and the local unemployment rate.

The main identifying assumption for the Differences-in-Differences estimator to identify the average treatment effect on the treatment group is parallel time trends for the treatment and control group in absence of the treatment. This assumption could be violated for at least three reasons. First, the composition of treatment and control group could change. Second, labor market outcomes might evolve differently across treatment and control groups because their outcomes respond differently to economic cycles. Third, the reform may change incentives to become unemployed leading to more treated individuals who enter unemployment after the reform. I now discuss each of these three threads to identification.

2.3.1 Composition of Treatment and Control Group

Table 2.2 contains selected descriptive statistics for treatment and control group before ($A_t = 0$) and after ($A_t = 1$) the reform. Column (5) contains unconditional Differences-in-Differences estimates. The top panel shows how earnings, employment and conditional earnings before unemployment entry change between treatment and control group. Yearly earnings increase from €19,711 to €21,018 for the control group and somewhat stronger

[8]Appendix figure 2.10 shows that unemployment duration is highly cyclical for recalled job seekers, which makes the identifying assumption hard to assess and questionable. Excluding firms with a high ex-ante probability to recall workers instead of recalled individuals would yield the same results.

for the treatment group from €20,081 to €22,320. The differential increase is positive and statistically significant indicating that treated individuals who enter after the reform have on average higher earnings.[9] All individuals entering unemployment come from employment. Employment days decrease by the same amount for both treatment and control group. Conditional earnings increase somewhat stronger for the treatment group; the difference is significant and drives the increase in earnings. Individuals in the treatment group differ from those in the control group in some predetermined characteristics. The most apparent change is that more immigrants populate the control group after the reform, which also explains the drop in experience of that group.[10] The share of younger individuals who are more likely to enter unemployment from white-collar jobs and more likely to be divorced is larger in the treatment group after the reform. Furthermore, treated individuals are more likely to come from the real goods sector and less likely to become unemployed in the construction sector.

With the exception of more low experience immigrants entering the control group after the reform, changes in the characteristics are economically small. The main difference, the relatively large inflow of immigrants, only has a minor impact on the main result as shown in the robustness section. From this perspective, treated individuals who enter unemployment after the reform appear positively selected. Column (5) shows unconditional Differences-in-Differences estimates. The positive effect on earnings may be driven by observable and unobservable characteristics. I now assess to what degree conditioning on observables influences the effect on earnings.

The question is what the difference is in pre-unemployment earnings conditional on covariates. To assess these differences, I estimate equation (2.2) for earnings in the 10 years *before* unemployment entry. Figure 2.1 depicts point estimates from these regressions. Shaded areas indicate 95% confidence intervals, the x-axis measures years relative to unemployment entry. The figure shows that pre-unemployment earnings conditional on covariates are slightly negative in years 9 and 10 before unemployment entry. Pre-unemployment earnings are zero in all other years. This result implies that the unconditional change in earnings between treatment and control group from Table 2.2 is fully explained by observable characteristics. While observables explain differences in pre-unemployment earnings, their influence on post-unemployment earnings is limited as shown in the robustness section.

[9] Average yearly earnings are somewhat below median earnings of €22,332 for similar workers in the year 1990. Median earnings of the full working population is €12,907 but the individuals eligible for the increase in PBD only have earnings above €12,824.

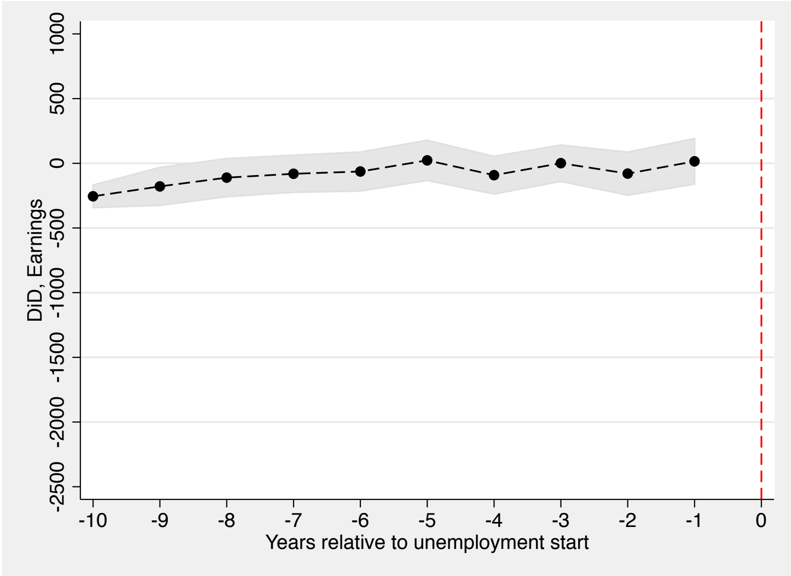
[10] Austria experienced a substantial influx of immigrants after the fall of the iron curtain in 1989.

Table 2.2: Selected Descriptive Statistics

	Control group		Treatment group		
Before/After	$A_t = 0$ (1)	$A_t = 1$ (2)	$A_t = 0$ (3)	$A_t = 1$ (4)	DiD (5)
Pre-UI earnings and policy intervention					
Yearly Earnings (EUR)	19,711	21,018	20,081	22,320	932.2***
Days employed	315.0	317.2	312.4	314.6	0.05
Yearly earnings (EUR) | employed	19,814	21,178	20,232	22,569	973.1***
PBD (weeks)	28.64	27.55	29.70	43.05	14.44***
Unemployment (weeks)	24.25	23.73	27.93	34.55	7.14***
Nonemployment (weeks)	50.17	60.98	63.53	89.74	15.40***
Control Variables					
Austrian	0.77	0.64	0.74	0.73	0.11***
Experience days last 5 years	1540.6	1408.1	1675.8	1704.6	161.3***
White Collar	0.27	0.27	0.29	0.35	0.06***
Age	38.31	39.45	46.50	47.27	-0.38***
Higher Education	0.13	0.15	0.12	0.15	0.01
Family status					
Single	0.16	0.20	0.09	0.10	-0.03***
Married	0.68	0.66	0.76	0.74	0.00
Divorced	0.16	0.14	0.14	0.15	0.03***
Widow	0.00	0.00	0.01	0.01	-0.00
Industry					
Agriculture	0.01	0.01	0.01	0.01	-0.00
Energy	0.00	0.00	0.00	0.00	-0.00
Mining	0.01	0.01	0.01	0.01	0.00
Real Goods	0.33	0.34	0.31	0.38	0.06***
Construction	0.26	0.30	0.21	0.18	-0.07***
Wholesale and Retail	0.15	0.16	0.17	0.20	0.02***
Tourism	0.05	0.03	0.06	0.04	-0.01
Transportation	0.05	0.05	0.06	0.06	0.01
Financial	0.06	0.05	0.07	0.06	0.00
Personal Services	0.01	0.01	0.02	0.01	-0.00
Arts and Sports	0.01	0.01	0.01	0.01	0.00
Health	0.01	0.00	0.02	0.01	-0.01***
Education	0.01	0.00	0.01	0.01	-0.00
Government	0.03	0.02	0.04	0.03	-0.01
Housekeeping	0.00	0.00	0.00	0.00	0.00
No. of observations	17,430	45,186	30,320	63,909	156,736

Notes: Table 2.2 shows means of selected variables for the treatment and control groups who registered before or after 1 August 1989, respectively. Column (5) contains unconditional DiD point estimates. Yearly earnings, days employed and conditional yearly earnings are measured in the year before UI inflow. Experience is measured in the 5 years before UI inflow. Earnings are deflated to the year 2000. White collar status and industry is measured on the last job before UI inflow. Significance is indicated as follows: * (p<0.1), ** (p<0.05), *** (p<0.01).

Figure 2.1: Treatment Effect for Pre-unemployment Earnings



Notes: The figure shows treatment effects for pre-unemployment earnings in each of the 10 years before unemployment entry. Dots depict point estimates, shaded areas depict 95% confidence intervals. Own illustration using data from ASSD.

2.3.2 Evolution Over Time

Whether the treatment group would have evolved differently over time than the control group in absence of the treatment cannot be tested directly. But the data allow following treatment and control groups over time. In particular I can assess whether there are differences between the two groups *before* the implementation of the reform. One test, proposed in Autor (2003), is implemented by means of the following modification of equation (2.2):

$$Y_{it} = \beta_0 + \sum_{\tau=t_0-k}^{t_0+p} \delta_\tau D_i \mathbb{1}(t_i = \tau) + \beta_1 D_i + \beta_2 A_t + \beta_\tau \mathbb{1}(t_i = \tau) + X'_{it}\beta + \varepsilon_{it} \tag{2.3}$$

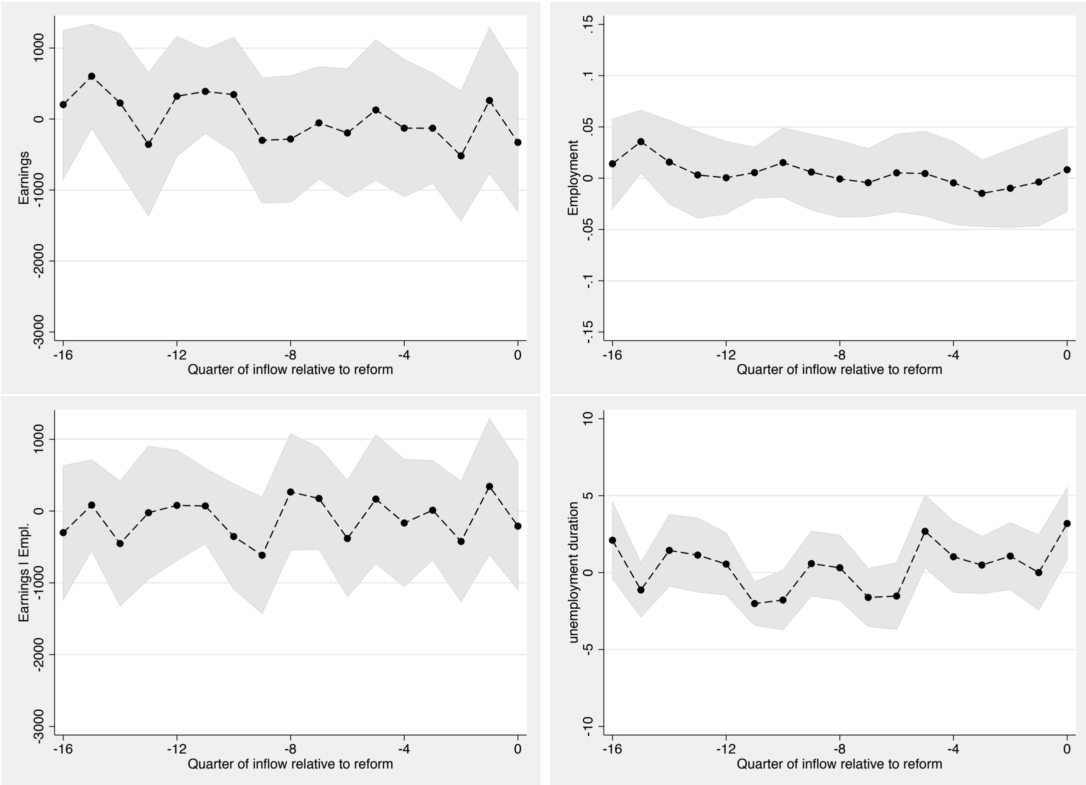
where t_0 denotes the reform date, τ measures the quarter of the inflow and runs from $k = 16$ quarters before the reform to $p = 24$ quarters after the reform. $\mathbb{1}(t_i = \tau)$ is a dummy variable for the quarter of inflow. δ_τ measures the treatment effect in quarter τ relative to anything specific to that quarter captured in β_τ, i.e. relative to the control group. The specification essentially splits the treatment effect δ from equation (2.2) to single quarters around the reform by allowing flexible group-specific changes over time. If no pre-existing differential trends appear between treatment and control groups, we can expect the coefficients interacted with the quarters before the reform to be zero and the time profile flat. Clear differences before the reform could be problematic as they would indicate that treatment and control groups had already evolved differently over time *before* the reform.

Figure 3.8 depicts the estimated coefficients $\hat{\delta}_t$ along with 95% confidence intervals for each of the 16 quarters before the reform. The figure displays point estimates for earnings measured in the year after unemployment entry (top left), employment status, conditional earnings and duration of initial unemployment (bottom right). The point estimates for earnings fluctuate around, and are close to zero. Only for inflow in quarter 15 before the reform is there a statistically significant positive effect. This positive effect is likely due to employment status that is significantly positive for this quarter of inflow (top right). Conditional earnings are not statistically significant in any of the quarters. There are no obvious trends in the outcomes before the reform. Furthermore, estimating the treatment effect only using inflow 8 quarters before the reform does not change the main result.

The increase in PBD also affects unemployment duration. Shifts in unemployment duration that are not related to the reform would violate the design similarly to non-parallel trends in earnings. The bottom right graph of the figure confirms that there is no differential in unemployment duration between treatment and control group before the reform. Point estimates fluctuate around zero, with one marginally negative effect 11 quarters before the reform. Eventually, increases in unemployment duration in the quarter leading to the reform (0 on the x-axis). This increase may occur either because of anticipation of the reform or because the reform also affects ongoing spells. A robustness check shows that excluding inflow up to one year before the reform does not change the results.[11]

[11] Appendix Figure 2.11 shows the evolution of these four outcomes over the entire estimation period.

Figure 2.2: Parallel Trends Before the Reform



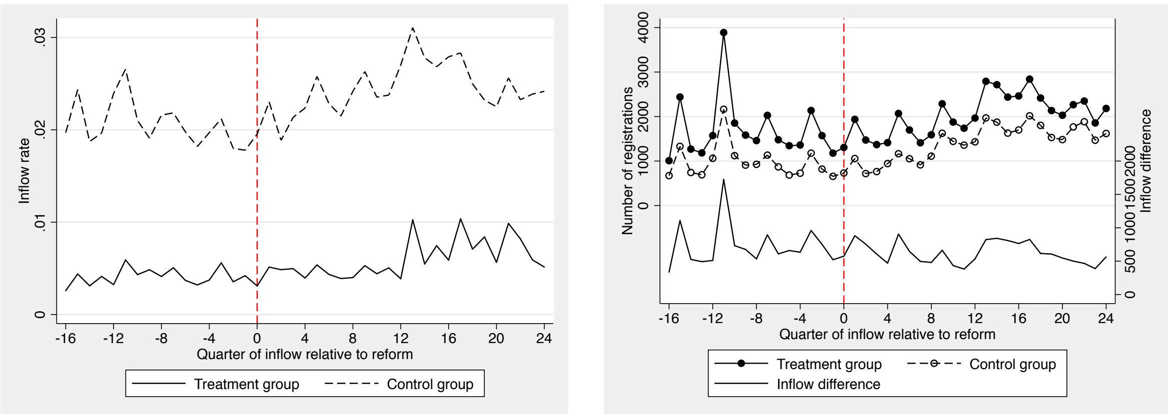
Notes: The figure shows how treatment effects evolve in the 16 quarters before the reform. Dashed lines are treatment effects interacted with indicator variables for quarter relative to treatment as in equation (2.3). Shaded areas depict 95% confidence intervals. Own illustration using data from ASSD.

2.3.3 Endogenous Entry

The third test I implement assesses endogenous entry into unemployment. If treated individuals enter unemployment insurance more frequently after the reform because they expect higher benefit duration, the estimated treatment effect would be biased (Lalive et al., 2011). To assess inflow, I relate the inflow into unemployment to the total population at risk to enter unemployment in either treatment or control group. Panel (a) of Figure 2.3 plots the inflow rates by treatment and control group. Inflow rates of the two groups evolve parallel. There is no discernible shift in the inflow rate of the treatment group after the reform. There is, however, a slight increase in the inflow rate for the control group starting from quarter 8 after the reform. As the reform increased attractiveness of unemployment for treated individuals, this result is surprising and reassuring at the same time. Endogenous entry as response to the reform is unlikely in this context. Panel (b) plots inflow levels by treatment status and confirms that there is no difference in inflow levels after the reform. Again, we see that the number of registrations increases starting from quarter 8 but the difference in registrations remains constant. A look at the graph indicates that endogenous entry in this context may not be a concern.

Figure 2.3: Endogenous Entry Into Unemployment

(a) Inflow relative to employment **(b)** Inflow level absolute



Notes: Subfigure (a) shows the inflow into treatment and control group as fraction of all employed worker eligible for either treatment or control. Subfigure (b) depicts the number of registrations for treatment and control group per quarter on the left axis. The solid line depicts the inflow difference between treatment and control group on the right axis. The dashed vertical line marks the reform date. Corresponding Differences-in-Differences estimates are in Table 2.3. Own illustration using data from ASSD.

Estimating the changes in inflow between treatment and control group reinforces the visual impression. Table 2.3 presents results from a regression of the inflow on the treatment dummy D_i, the interaction D_iA_t and a set of dummies for each quarter. On average, the treatment group has a lower inflow rate (D_i). As the visual inspection as suggested, the inflow rate is lower for the treatment group compared to the control group (column (1)). It is safe to say that there are not more treated individuals entering unemployment after the reform. The same is true when assessing the inflow levels (column (2)). Endogenous entry in the treatment group is not a concern in this context.

Table 2.3: DiD Estimates for Endogenous Entry Into Unemployment

	Inflow rate (1)	Inflow level (2)
D_iA_t	-0.002**	-95.14
	(0.001)	(84.51)
D_i	-0.017***	–240.0***
	(0.000)	(25.86)
Time Fixed Effects	Yes	Yes
Obs.	82	82
Adj R2	0.985	0.921

Notes: Table 2.3 shows DiD estimates for difference in the inflow. Column (1) reports estimates for the difference in the inflow rate corresponding to Panel (a) of Figure 2.3. Column (2) reports estimates for the difference in the inflow level corresponding to Panel (b) of Figure 2.3. Robust standard errors in parentheses. Significance is indicated as follows: * (p<0.1), ** (p<0.05), *** (p<0.01).

Thus the assumptions on parallel trends are likely to be satisfied.

2.4 Results

This section presents the results of the effects of the increase in PBD. First, I discuss the results on earnings, employment status and earnings conditional on employment. Second, I assess robustness of the main results and decompose the effect on earnings. Third, I assess the earnings response over time by focusing on single years and then break down nonemployment into various social benefits and inactivity.

2.4.1 The Effect of PBD on Earnings

Table 2.4 reports estimates for the cumulative loss over ten years in earnings. As a consequence of the reform, treated individuals experience a loss in earnings of €6,927 over the ten years after unemployment entry. Evaluated at the sample mean, earnings of the treatment group are 6 percent lower than those of the control group. Per week of PBD increase, treated individuals have €481 lower earnings over 10 years, or €48.1 on average per year. Degen and Lalive (2015) study a decrease in PBD of 6 months and report an *in*crease in earnings of 0.156 percent per week of PBD *de*crease over a four-year period. The difference in the results either is because of non-symmetric effects of changes in PBD. Or the effect of PBD on post-unemployment outcomes is non-linear. Contrarily, Kolsrud et al. (2015) document a drop in consumption of about 18 percent 1 year after unemployment entry, which is related to lower earnings. Their estimated effect is larger than mine because they compare consumption (and earnings) before and during unemployment and not between treatment and control group. The estimated difference in earnings can be decomposed into employment status and conditional earnings. Both are lower in this context contributing to lower earnings for the treatment group. On average, treated individuals are 3.2 percentage points less likely to have a job over the 10 years after unemployment entry (column (2)). The magnitude of the effect is similar to what Degen and Lalive (2015) estimate over a four-year period (3.3 percentage points). The absolute loss in conditional earnings because of the reform amounts to €4,884 (column (3)), a loss that constitutes of fewer employment days and lower daily earnings.

The question arises why earnings are lower for the treatment group compared to the control group. Given the theoretical considerations outlined in the beginning, lower earnings are the consequence of lower job match quality, itself being related to increased non-employment duration as a consequence of the policy change. The increase in nonemployment duration is also likely to drive down job-match quality: the response in nonemployment duration is relatively large. On average, individuals are allowed to spend 14.4

weeks longer in UI (see Table 2.2). The implied marginal effect for nonemployment duration is 0.45 or 13.5 days per month of PBD extension.[12]

Typical estimates for European studies range from 0.05 to 0.65 with a mean marginal effect of 0.23 (Schmieder and von Wachter, 2016). Two studies explicitly relate nonemployment duration to re-employment wages. Schmieder et al. (2016) estimate a marginal effect for nonemployment duration of 0.16 and a negative effect on re-employment wages of 1.2 percent. In contrast, Nekoei and Weber (2015) estimate a marginal effect for nonemployment duration effect of 0.02 and a positive effect on re-employment wages of 0.4 percent. Thus, as the results here suggest, a higher marginal effect on nonemployment duration leads to worse post-unemployment outcomes.

Table 2.4: DiD Estimates: Lower Earnings After Longer Initial Unemplyoment

	Earnings	Empl.	Earnings | empl.	nonempl. duration (weeks)	unempl. duration (weeks)
	(1)	(2)	(3)	(4)	(5)
DiD	-6,927***	-0.032***	-4,884***	6.427***	4.470***
	(1,121.4)	(0.004)	(1,117.0)	(0.607)	(0.284)
Controls	Yes	Yes	Yes	Yes	Yes
Mean Y	115,183	0.834	138,041	35.719	27.933
Obs.	156,728	156,728	128,843	130,436	156,728
Adj R2	0.218	0.171	0.226	0.101	0.159
Rel. to sample mean (%)	6.014	3.837	3.538	17.99	16.00
Per week of PBD	481.0	0.002	339.2	0.446	0.310

Notes: Table 2.4 contains DiD estimates. Outcomes in columns (1)-(3) are measured cumulative over the 10 years after UI inflow. The outcome *Empl.* is measured as being employed at least once in the 10 years. *Mean Y* refers to the mean outcome in levels of the treatment group before the reform. The full set of controls is used including the local unemployment rate, daily wage and tenure of the last job, experience in the last 5, 10 and 15 years, earnings and unemployment days in the 10 years before unemployment inflow, indicator variables for industry, region, year, month, age, family status and education, higher order polynomials of tenure, last daily wage and all experience measures. Standard errors are in brackets and clustered on individual level. Significance is indicated as follows: * (p<0.1), ** (p<0.05), *** (p<0.01).

2.4.2 Sensitivity Analysis

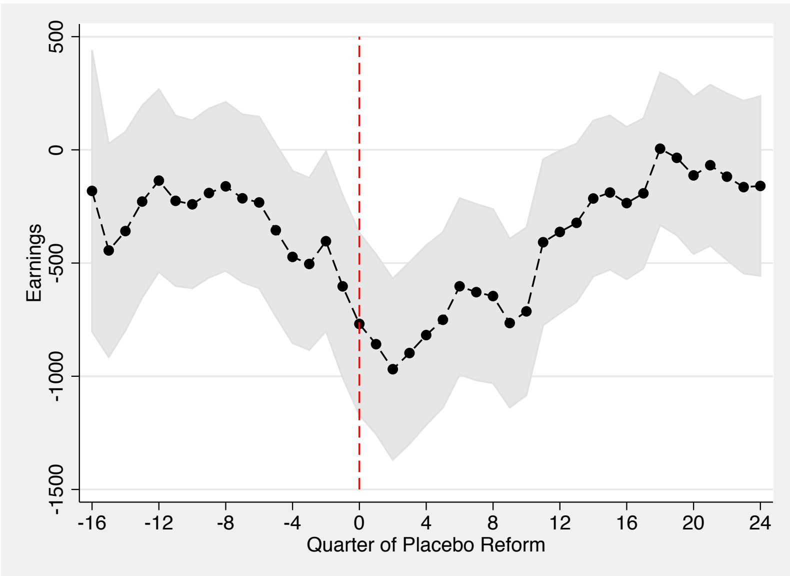
Before continuing the analysis, I examine the remaining concerns from the section on identification and also assess robustness of the results. First, I implement a series of placebo estimations to check whether trends were indeed parallel before the reform was implemented. To do this, I simulate placebo reforms starting in August 1985 and shift the reform date in 90-day steps up to August 1996. For each placebo reform I use ± 3 years

[12]Lalive et al. (2006) report an elasticity of 0.085 for the same reform. After I replicate their sample by including again recalled job seekers and also women, the elasticity becomes 0.11. Indeed, most of the reduction is due to recalls.

of inflow around each placebo reform date. I use earnings in the year after the reform because of repeated unemployment.

Figure 2.4 shows the point estimates for each single placebo reform. The x-axis measures time of the placebo reform relative to the actual reform date. Before quarter -8 there is no effect of this placebo reform on earnings because inflow is always before the actual reform. Despite substantial variance, there only is a significant effect of the reform in quarters larger than -6. The effect becomes stronger as more and more individuals who are affected by the reform enter the estimation sample. The effect remains negative some quarters beyond the reform, eventually because of worsening economic conditions as seen in the assessment of the inflow. However, the placebo effects fade out after the estimation window is shifted beyond the actual reform because only observations entering unemployment after the actual reform date remain in the estimation sample.[13] The reform is likely to shift earnings. Insignificant treatment effects if only inflow before the reform is considered suggests that the evolution of treatment and control group is similar in absence of the treatment.

Figure 2.4: Placebo Reforms for Earnings



Notes: Figure 2.4 shows point estimates from Differences-in-Differences regressions for a series of placebo reforms. Each placebo reform uses ± 3 years of inflow. Placebo reform dates start in August 1985 and shift every 90 days up to August 1996. The x-axis measures quarters relative to the actual reform date. Shaded areas depict 95% confidence intervals. The outcome is earnings in the year after unemployment entry. See the text for further details. Own illustration using data from ASSD.

The next question is what the influence of heterogeneity is on the results. To investigate their robustness, I repeat the analysis for various subsamples closer to the policy intervention thresholds in terms of age, experience and calendar time. Panel A of Table

[13] A similar picture results for unemployment duration, see Appendix Figure 2.12.

2.5 contains the baseline estimates from Table 2.4 for ease of exposition. By definition of the treatment, treated individuals are on average older and have more work experience. Despite holding constant these characteristics in the estimation, the estimated treatment effect could be the result of these differences because of insufficient overlap in control variables. The following robustness checks also help to alleviate these concerns. Panel B restricts age at inflow to 45 or younger leaving out old individuals. The treatment effect becomes weaker but remains negative and significant; the increase in PBD still leads to a decrease in earnings. Younger job seekers appear less responsive to the reform than older job seekers, particularly regarding employment status. Reasons may include different treatment intensity or different behavioral responses. I assess differences by age in more detail below.

Panel C presents estimates with the sample restricted to inflow of high experience individuals. This variation in the sample mainly cuts down on the control group. If the results would change substantially, then the choice of the control group would be a concern. The treatment effect becomes somewhat stronger but still remains close to the baseline estimate suggesting that heterogeneity in experience does not largely influence results.

In panel D, I narrow the time window and only use inflow $\pm$ 2 years around the reform date instead from August 1985 to August 1996. Reducing the time window around the reform reduces the influence of differing economic conditions over time. The effect on earnings is about 30 percent smaller but still within range of the baseline. While the effect on conditional earnings is similar, mainly the lower effect of employment contributes to the different point estimate in earnings. Overall, significance is lower with this restriction because only 40 percent of the sample is used. Changing the control group or making both the control and the treatment group more similar in various ways does not largely influence the result.

As mentioned earlier, the composition of the inflow changed. There is an inflow of low-experienced immigrants to the control group after the reform. This change in the composition may be due to the general inflow of immigrants to Austria around the time of the reform and not because of the reform. But excluding immigrants from the analysis sheds light on the robustness of result with respect to changes of inflow composition.

Panel E presents regression results when immigrants are excluded from the analysis. The point estimate for earnings becomes slightly more negative in this case and similar to the one in Panel C where the sample was restricted to high experience individuals as probably similar individuals are excluded. Although the effect for employment differs the main result remains the same. Contrarily, only analysing immigrants reveils that their unemployment duration responds much stronger, while their earnings response becomes a bit smaller, comparable to younger job seekers (Panel B). The employment response of immigrants is somewhat stronger while there is not effect on conditional earnings.

The assessment of parallel trends showed an increase in unemployment duration for the treatment group for individuals entering unemployment in the quarter before the reform. Because the reform affected ongoing spells but the treatment is defined at unemployment entry, some individuals in the control group may be misclassified, which is likely to lead to an underestimation of the effect. Panel F contains results if inflow in the 12 months before the reform is excluded. This restriction ensures that no individual who entered unemployment before the reform can be eligible for the increase in PBD. Point estimates become somewhat more negative but the change is small compared to the baseline. Wrongly accounted spells to the control group despite the fact that they belong to the treatment group do only have a minor influence on the result.

The discussion so far was silent to what extent results are influenced by some form of observed or unobserved heterogeneity. In case treated individuals who enter unemployment after the reform are positively selected, the presented estimates are lower bounds. In case of negative selection, the presented estimates are upper bounds. Given higher unconditional pre-unemployment earnings for the treatment group relative to the control group, positive selection is likely in this context.

Oster (2013) presents a formal test for the influence of unobserved heterogeneity based on the movement of R^2 by inclusion/omission of control variables. The test therefore also sheds further light on the direct influence of control variables, which might be a concern because of insufficient overlap in control variables between treatment and control group. The test requires an assumption on the maximum R-squared, $\tilde{R}^2$, that can be reached in a given context. Oster recommends using 1.3 times the R^2 from the specification with control variables. The test then asks how much more important does unobserved relative to observed heterogeneity needs to be to yield a point estimate of zero to reach $\tilde{R}^2$. In principle, the test statistic can take any value from minus to plus infinity. A test statistic greater than one implies that unobserved heterogeneity must be more important than observed heterogeneity. A test statistic between zero and one would be of concern, because relatively little unobserved heterogeneity would induce the point estimate to become zero. A negative test statistic implies that unobserved heterogeneity would have to drag the point estimate in the opposite direction from observed heterogeneity.

I executed this test for earnings, conditional earnings, and employment status. Detailed results are available in Panels B, C and D of Appendix Table 2.8. The test statistic is mostly negative for earnings and conditional earnings implying that point estimates are bounded away from zero under the assumption of the test. One exception is earnings in year 9 after unemployment entry where the statistic is 0.380. For the outcome em-

Table 2.5: DiD Estimates: Sensitivity Analysis

	Earnings	Empl.	Earnings | empl.	nonempl. duration (weeks)	unempl. duration (weeks)
	(1)	(2)	(3)	(4)	(5)
A. Baseline	-6,927.1***	-0.032***	-4,883.8***	6.427***	4.470***
	(1,121.4)	(0.004)	(1,117.0)	(0.607)	(0.284)
Controls	Yes	Yes	Yes	Yes	Yes
Mean Y	115,183	0.834	138,041	35.719	27.933
Obs.	156,728	156,728	128,843	130,436	156,728
B. Only age 35-45	-4,185.7***	-0.006	-3,434.4**	4.677***	2.719***
	(1,459.8)	(0.004)	(1,405.3)	(0.746)	(0.345)
Controls	Yes	Yes	Yes	Yes	Yes
Mean Y	141,045	0.909	155,199	36.745	25.192
Obs.	91,091	91,091	82,360	83,733	91,091
C. High Experience	-7,689.2***	-0.037***	-5,836.1***	5.323***	4.012***
	(1,247.0)	(0.004)	(1,221.9)	(0.641)	(0.300)
Controls	Yes	Yes	Yes	Yes	Yes
Mean Y	115,160	0.834	138,023	35.733	27.942
Obs.	131,388	131,388	107,052	108,378	131,388
D. Restricted years of inflow	-4,378.8**	-0.014**	-4,333.2**	3.334***	3.436***
	(1,948.3)	(0.007)	(1,952.4)	(1.083)	(0.519)
Controls	Yes	Yes	Yes	Yes	Yes
Mean Y	115,376	0.806	143,145	36.123	27.436
Obs.	40,124	40,124	33,241	33,646	40,124
E. Only Austrians	-7,608.6***	-0.029***	-5,763.3***	4.575***	3.849***
	(1,306.4)	(0.004)	(1,303.4)	(0.712)	(0.335)
Controls	Yes	Yes	Yes	Yes	Yes
Mean Y	118,334	0.843	140,342	36.472	28.511
Obs.	110,907	110,907	91,488	92,905	110,907
F. Only Immigrants	-4,208.4*	-0.040***	-1,327.1	10.365***	5.448***
	(2,345.2)	(0.009)	(2,331.2)	(1.261)	(0.561)
Controls	Yes	Yes	Yes	Yes	Yes
Mean Y	102,280	0.796	128,437	33.740	26.282
Obs.	41,374	41,374	33,829	33,994	41,374
G. No inflow 1 year before reform	-7,233.0***	-0.035***	-5,127.1***	6.418***	4.649***
	(1,202.8)	(0.004)	(1,193.8)	(0.648)	(0.309)
Controls	Yes	Yes	Yes	Yes	Yes
Mean Y	115698	0.842	137348	35.769	28.129
Obs.	146,222	146,222	120,031	121,526	146,222

Notes: Table 2.5 contains DiD estimates for various sample restrictions for cumulative outcomes over 10 years after unemployment inflow. The results for single years is available in Appendix Table 2.7. Panel A corresponds to estimates in Table 2.4. Panel B focuses on a sample where individuals are between 35 and 45 years old at inflow. Panel C focuses on a sample of individuals with experience of at least 9 out of the last 15 years and 6 out of the last 10 years (also see Table 2.1 for the definition of the treatment). Panel D uses only inflow from the years 1987 to 1991 instead of 1985 to 1996 as in Panel A. Panel E uses only inflow of Austrians, Panel F uses only inflow of Immigrants. Panel G excludes inflow from the 12 months leading to the reform. *Mean Y* refers to the mean outcome in levels of the treatment group before the reform. The full set of controls is used (see notes of Table 2.4). Standard errors are in brackets and clustered on individual level. Significance is indicated as follows: * ($p<0.1$), ** ($p<0.05$), *** ($p<0.01$).

ployment status, the test statistic is at least 2.75. In this case, unobserved heterogeneity would have to be at least 2.75 times as important as observed heterogeneity to induce a point estimate of zero. Overall, the test suggests that the results are not susceptible to unobserved heterogeneity and point estimates are bounded away from zero.

The treatment effect is robust to a variety of changes in the sample, and the point estimates remain within reach of the baseline estimates. Unobserved heterogeneity does not play a key role beyond observed heterogeneity. Although the exact size of the point estimates varies with subsamples, results suggest a robust negative effect of an increase in PBD on post-unemployment earnings.

2.4.3 Decomposing Earnings

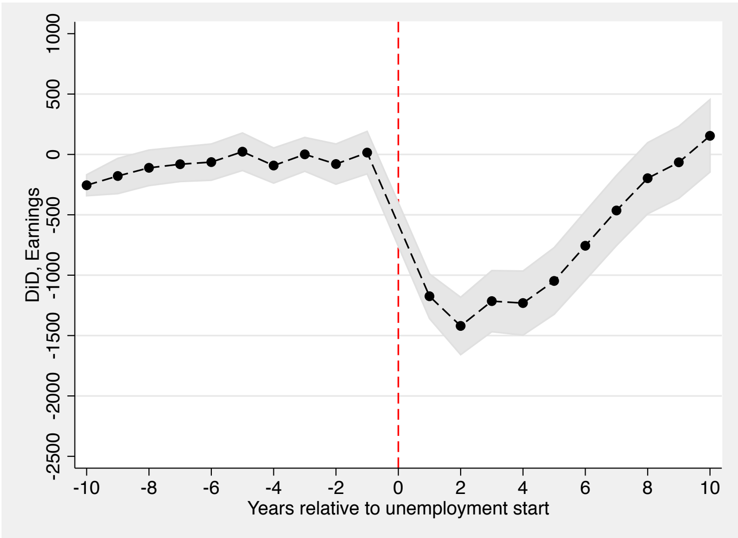
Earnings are a summary measure for the labor market situation of an individual in general. Decomposing earnings into their components yields a detailed analysis of changes in the underlying variables. The data set allows me to decompose earnings along several dimensions. First, I want to assess the adjustments of earnings, employment status and conditional earnings over time. The decomposition over time is instructive as it allows studying the timing of the effect. Furthermore, I can disentangle short-term effects directly related to the increase in initial nonemployment from long-term effects. Second, I can decompose conditional earnings into employment days and daily earnings. This decomposition yields a more detailed picture of different measures for job quality. Third, employment is mirrored by nonemployment within a year. Thus, decomposing nonemployment into various different components is important for understanding long-term consequences of the increase in PBD on other social benefits.

Evolution Over Time. Figure 2.5 presents graphical results for earnings, employment status and conditional earnings for each year around unemployment entry.[14] The figure shows point estimates from OLS regressions of equation (2.2) on earnings in each of the 10 years before and after unemployment entry. The x-axis is centered on unemployment entry. There is no discernible difference in earnings before unemployment entry as already seen in Figure 2.1. Earnings for the treatment group drop by €1,175 (about 17 percent of the effect over 10 years) in the year following unemployment entry because of the increase in PBD. This loss in earnings is directly linked to the increase in initial nonemployment due to the increase in PBD and therefore part of the behavioral response during *initial* unemployment. Two years after unemployment entry, the earnings difference remains low with €1,406. This point estimate is not a direct result of increased unemployment but an effect beyond the initial unemployment period. The effect on earnings remains on this low level through year 4 after unemployment entry and starts to approach zero afterwards. In year 8 and beyond the point estimates are not statistically significant from zero implying a zero difference in earnings between treatment and control group in these years. While gap in yearly earnings closes it also implies that the the treatment group is

[14]Corresponding regression results for post-unemployment years are available in Appendix Table 2.9.

worse off.[15] This result is related to Kolsrud et al. (2015) and Degen and Lalive (2015) insofar as there appears to be a more or less permanent effect on earnings during the first four years. However, the analysis beyond year four reveals adjustments that do not allow me to conclude that changes in PBD have a permanent effect on earnings.

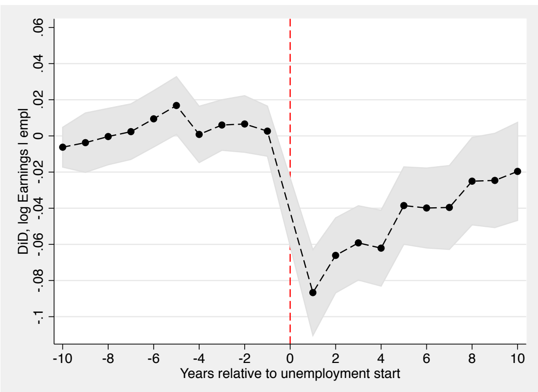
Figure 2.5: Treatment Effect for Earnings



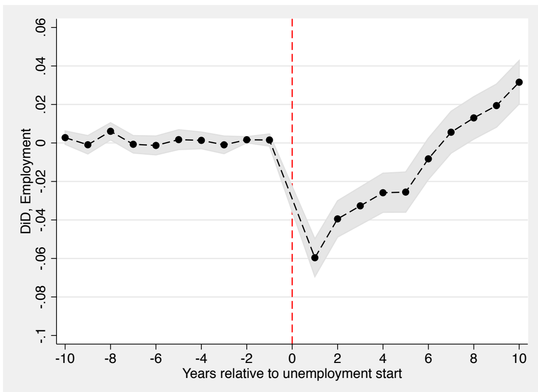
Notes: The connected dots in Figure 2.5 depict the treatment effects for each year before and after unemployment entry (x-axis). Earnings are measured in levels. The full set of control variables is used for the regressions (see notes of Table 2.4). Shaded areas correspond to 95% confidence intervals. The corresponding regression results are in Appendix Table 2.9. Own illustration using data from ASSD.

Figure 2.6: Treatment Effect for Conditional Earnings and Employment

(a) Conditional Earnings



(b) Employmnet



Notes: The connected dots in Figure 2.6 depict the treatment effects for each year before and after unemployment entry (x-axis). Employment is measured as one if an individual has positive earnings in that year, conditional earnings are measured in logs. The full set of control variables is used for the regressions (see notes of Table 2.4). Shaded areas correspond to 95% confidence intervals. The corresponding regression results are in Appendix Table 2.9. Own illustration using data from ASSD.

[15]Indeed, major movements in the Differences-in-Differences estimates over years after unemployment entry stem from level differences between treated individuals entering before/after the reform, where treated individuals entering before the reform show a stronger decline in earnings over time.

Earnings can be low either because workers are not employed or because they earn less while employed. Panel (a) in Figure 2.6 depicts point estimates from the regressions with log conditional earnings as outcome. Like earnings, point estimates for pre-unemployment conditional earnings are close to zero, dropping by about 8 percent with unemployment entry because of the reform. While point estimates gradually decline, they remain at minus 2 percent in year 10 after unemployment entry but are not sufficiently precisely estimated to be significantly different from zero. The decrease in point estimates for conditional earnings contributes to the decrease in point estimates for earnings. Finally, Panel (b) shows treatment effects for employment status. The general pattern is similar to Panel (a) no discernible difference in employment status before unemployment entry. With unemployment entry, treated individuals experience 6 percentage point lower employment in the year following unemployment entry. The effect gradually approaches zero afterwards. In year 6 after unemployment entry there is no difference in employment status. With year 7, the treatment group shows higher employment compared to the control group. The positive effect after year 6 emerges because a large part of older treated individuals are less likely to permanently leave employment (see below). Similar to conditional earnings, the movements in employment contribute to the adjustments in earnings. Both parts, employment status and conditional earnings contribute to the initial drop in earnings but also to the decline in the effect over time. Increasing PBD has long-term consequences beyond the effect on the initial duration of nonemployment – and likely beyond the first job after the unemployment spell as well.

Conditional Earnings. Using the definition from equation (2.1) I decompose conditional earnings into employment days and daily earnings, thereby obtaining a clearer picture of the influence of daily earnings or employment days on conditional earnings and of which component drives the adjustments over time. Furthermore, the decomposition also allows for a closer comparison of the size of the point estimates to recent studies. Table 2.6 presents results for the decomposition of conditional earnings in each of the ten years following unemployment entry. Panel A contains point estimates from equation (2.2) for log conditional earnings, Panel B for log employment days, and Panel C for log daily earnings.

For employed individuals in the year following unemployment entry, only a small negative difference appears in log daily earnings and it is not statistically significant. The difference increases in year two and remains negative throughout the following years. On average treated individuals have lower daily earnings of between 1.4 to 3.9 percent or between 0.097 and 0.271 percent for each week that PBD increases. The effect on daily earnings is similar to Schmieder et al. (2016) in two distinct ways. First, the effect size is

Table 2.6: Decomposition of Conditional Earnings

Year after UI start	1	2	3	4	5	6	7	8	9	10
A. ln Earnings | empl.	-0.087***	-0.066***	-0.059***	-0.062***	-0.039***	-0.040***	-0.040***	-0.025**	-0.025*	-0.020
	(0.012)	(0.011)	(0.010)	(0.011)	(0.011)	(0.011)	(0.012)	(0.012)	(0.013)	(0.014)
Mean Y	9.210	9.600	9.680	9.720	9.750	9.763	9.775	9.769	9.754	9.748
N	104,485	111,162	107,901	103,545	99,033	94,052	88,616	83,099	77,594	72,103
B. ln empl. days | empl.	-0.083***	-0.052***	-0.038***	-0.037***	-0.016*	-0.013	-0.000	0.011	0.013	0.017
	(0.011)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)	(0.011)
Mean Y	5.173	5.540	5.593	5.613	5.616	5.611	5.606	5.594	5.580	5.577
N	104,485	111,162	107,901	103,545	99,033	94,052	88,616	83,099	77,594	72,103
C. ln daily earnings	-0.004	-0.014***	-0.022***	-0.025***	-0.022***	-0.027***	-0.039***	-0.036***	-0.038***	-0.037***
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)
Mean Y	4.04	4.06	4.09	4.11	4.13	4.15	4.17	4.18	4.17	4.17
N	104,485	111,162	107,901	103,545	99,033	94,052	88,616	83,099	77,594	72,103

Notes: Table 2.6 contains DiD estimates for the decomposition of log conditional earnings (Panel A) into log employment days (Panel B) and log daily earnings (Panel C). Column 1 contains results when the outcome is measured in the first year after unemployment entry. Column 2 contains results when the outcome is measured in the second year after unemployment entry and so forth. *Mean Y* refers to the mean of the respective outcome for the treatment group before the reform. The full set of controls is used (see notes of Table 2.4). Standard errors are in brackets and clustered on individual level. Significance is indicated as follows: * ($p<0.1$), ** ($p<0.05$), *** ($p<0.01$).

comparable: they estimate a decline in re-employment wages of 0.20 percent per week of PBD increase. Second, the difference in wages can be the result of dynamic selection of workers over the nonemployment spell.

Appendix Figure 2.13 shows that conditional on nonemployment duration, there is no difference in post-unemployment daily earnings. Dynamic selection therefore is not an issue here. This result implies that the reform did not shift reservation wages upward. Instead, the effect of the increase in PBD on daily earnings arises through increased nonemployment durations.[16] Permanently lower daily earnings may be the result of a higher share of treated individuals working part time after unemployment. Data limitations do not allow for a further decomposition in the wage rate or hours worked. Adjustments in employment add to the relatively moderate effect on daily earnings resulting in the relatively large effect on earnings.

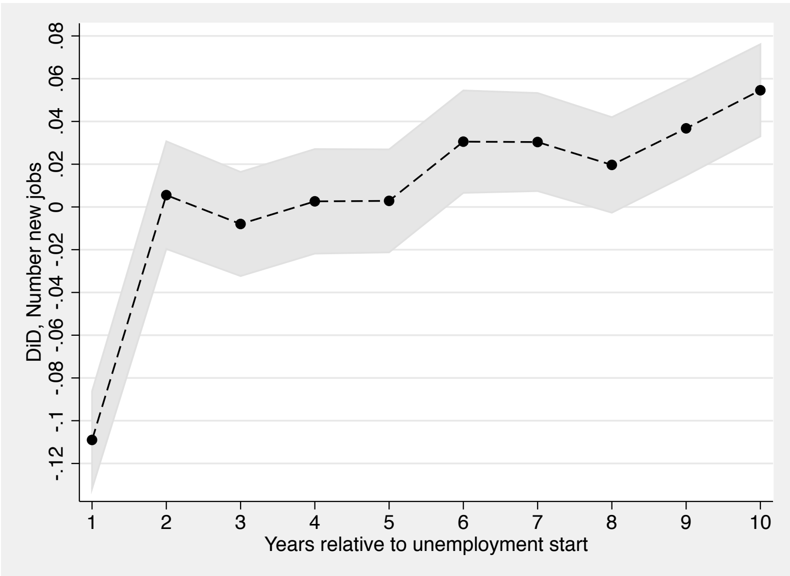
Panel C reports treatment effects for employment days conditional on employment. The reduction in employment days accounts for over 90 percent of the decline in conditional earnings in the first year. Treated individuals work fewer days if they are employed compared to individuals from the control group which partly is a direct consequence of longer initial nonemployment. But even in year two after unemployment entry, treated individuals work on average 5.2 percent fewer days compared to controls. Over time, the treatment effect on conditional employment days gradually approaches zero and is statistically insignificant in year 6 after unemployment entry. It likely turns positive afterwards. Negative effects in both daily earnings and conditional employment days imply on average worse jobs for treated individuals in both job match quality dimensions. The decline in the effect on conditional employment days indicates that job quality is improving for the treatment group. Furthermore, this improvement compensates for the permanently lower daily earnings contributing to the decline in the effect on conditional earnings and earnings.

Measured by both daily earnings and employment days, treated individuals get worse jobs as a consequence of staying unemployed longer but improve their situation over time. Another widely used measure for job quality is the duration of post-unemployment jobs. Short post-unemployment tenure or equivalently frequent job changes would indicate that workers try to leave their current job thus finding a better job-match, which improves their situation (Jovanovic, 1979; Burdett and Mortensen, 1998). For all individuals who successfully found a job after unemployment, I construct a variable indicating the start of a new job in a given year. Thus, the variable measures the extent to which employed individuals change employers. Figure 2.7 presents point estimates for job changes for

[16] Lalive et al. (2016), studying a reform in Austria, also find that the re-employment wage path is unaffected by the PBD extension.

each year after unemployment entry. Treated individuals are much less likely to start new jobs in the first year after unemployment entry essentially because they have no jobs in the first place due to prolonged nonemployment. Therefore, they cannot change jobs very frequently. No apparent differences in job changes emerge in years 2 through 5 after unemployment entry. Starting with year 6, individuals from the treatment group increasingly leave the current employer to find a new and possibly better job match. Accepting a low-quality job after unemployment and changing jobs afterwards appears to be a relevant margin of adjustment. Finding better jobs over time at least partly explains why the earnings gap decreases over time.

Figure 2.7: Job Changes Conditional on Employment



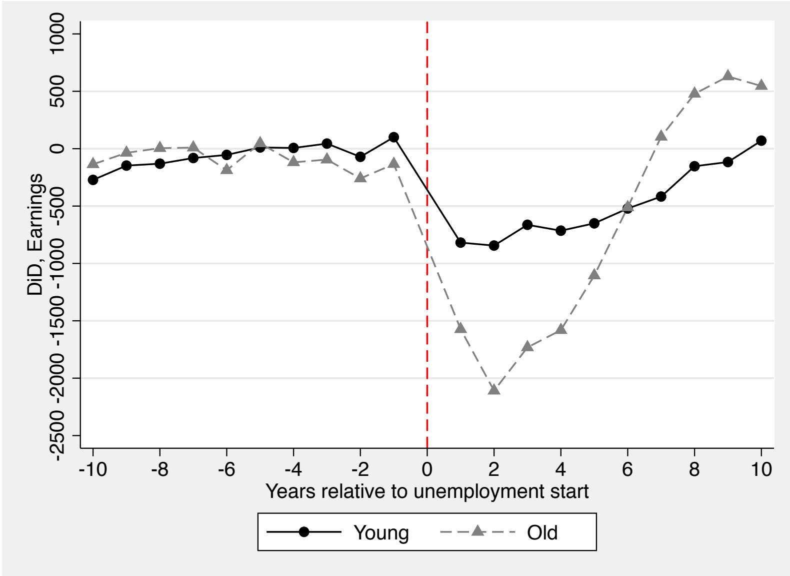
Notes: Figure 2.7 shows treatment effects for job changes by year after unemployment entry. Each point corresponds to a single regression. The outcome variable is one if the individual starts a new job in a given year. The variable is zero for the first job after unemployment. The full set of control variables is used in the regressions (see notes of Table 2.4). Own illustration using data from ASSD.

2.4.4 Treatment Effect Heterogeneity by Age

Adjustments in employment are an important driver for the effect of earnings over time. Both conditional employment days and employment status contribute to the absolute decline in the earnings difference between treatment and control group. Adjustments in employment take place very slowly and many individuals experience lower employment beyond the period of initial unemployment benefit receipt. Unsuccessful job seekers remain nonemployed. The question is what happens to individuals who are not working or not working all year long. To answer this question I break down nonemployment into various states of nonemployment beyond unemployment insurance including unemployment assistance, disability, old-age and sickness. Individuals may also be unobserved in the data for various reasons for which case I label their status as "inactive". Breaking down nonemployment is particularly important for older individuals. Particularly in Austria, unemployed individuals above age 54 have eased access to early retirement in the form of disability insurance. Inderbitzin et al. (2016) show that extended unemployment insurance can substitute for other social benefits. It can also complement them if individuals use extended unemployment to bridge the time to other social benefits.

To separate adjustments stemming from the reform and from potential program interaction effects, and because the treatment also differs by age, I split the treatment group into individuals aged 49 or younger and aged 50 and older. As mentioned earlier, individuals younger than 50 were eligible for an extension in PBD from 30 to 39 weeks, whereas job seekers aged 50 and older were eligible for an extension from 30 to 52 weeks. Under monotonicity of the treatment effect, we could expect the earnings response of older individuals to be stronger. Figure 2.8 plots the effect for earnings by the two age groups. Younger individuals show a weaker response in earnings, about only half the size of the response of older individuals. Yet, the estimated effect is still substantial and significant suggesting that treated individuals find worse jobs. Interestingly, the earnings difference for younger job seekers steadily converges back to zero. Returning to work is the single most important outcome after unemployment, hence once back to employment earnings differences vanish. However, the treatment group does not overtake the control group in terms of earnings and there remains a gap in earnings measured over 10 years. Conversely, the difference for older individuals turns positive after year 7. The decision to return to work is potentially much different for older job seekers as they have various alternatives to stay nonemployed.

Figure 2.8: Treatment Effects by Age for Earnings



Notes: Figure 2.8 shows treatment effects for earnings in each year before and after unemployment entry by age group. Young consists of treated individuals aged 40 to below 50, old uses treated individuals aged 50 and older. The full set of control variables is used in the regressions (see notes of Table 2.4). Own illustration using data from ASSD.

Changes in Nonemployment by Age. Figure 2.9 shows nonemployment decomposed into various nonemployment states over the 10 years after unemployment entry by age group. First of all, nonemployment can imply that job seekers are unemployed. Either job seekers draw on unemployment insurance (UI) or unemployment assistance (UA). From the top two graphs in Figure 2.9, it becomes clear that the response of younger job seekers in either UI or UA is weaker than that of older job seekers. Furthermore, older job seekers rely more heavily and more permanently on UA and are repeatedly unemployed starting from year 7 after unemployment entry.

Overall, older treated job seekers remain in the unemployment system for an extended amount of time due to the reform and far beyond PBD. The incidence of disability (DI) or old-age (OA) pensions and also sick leave or inactivity is almost not existent for younger job seekers as shown in the left graph of the second row. This is much different for older job seekers. Starting with year four, treated individuals have a lower incidence of days in disability and over time spend fewer days on old-age pensions. Both inactivity and sick leave show minor responses. The response on both UI and UA may indicate program substitution in particular in the short run as a direct consequence of the reform. The results on DI and OA point in the same direction: treated individuals remain on UI or UA and are therefore less likely to enter DI or OA. However, treated individuals show higher employment beyond year 7 (lower days in old-age pension do not compensate for higher unemployment).

There are several candidates to explain why older treated individuals would have higher employment and ultimately higher earnings after year 7. First, lower initial earnings as a consequence of the reform can have two effects. One is a pure income effect, which induces higher employment among treated individuals. The other is an anticipation effect because lower earnings and employment translates into lower pension payments. A forward-looking agent would want to increase earnings to not forgo pension payments. Both hypotheses remain suggestive, as there is no data to test them. Second, the pattern could be the consequence of disability and old-age reforms. Staubli (2011) studies a reform in 1996 that increased the age from 55 to 57 at which individuals have eased access to disability. Staubli and Zweimüller (2013) study how the increase in early retirement age from 2001 onwards affects labor market participation and unemployment. Both reforms could in principle have an impact on my results insofar as it could be that I compare treated individuals who no longer have eased access to either DI or OA but remain in the labor force. Accounting for the fact that certain individuals are no longer eligible for such eased access has a clear effect on days in disability but virtually no effect on days in old-age pension. Importantly, earnings are not affected neither.[17]

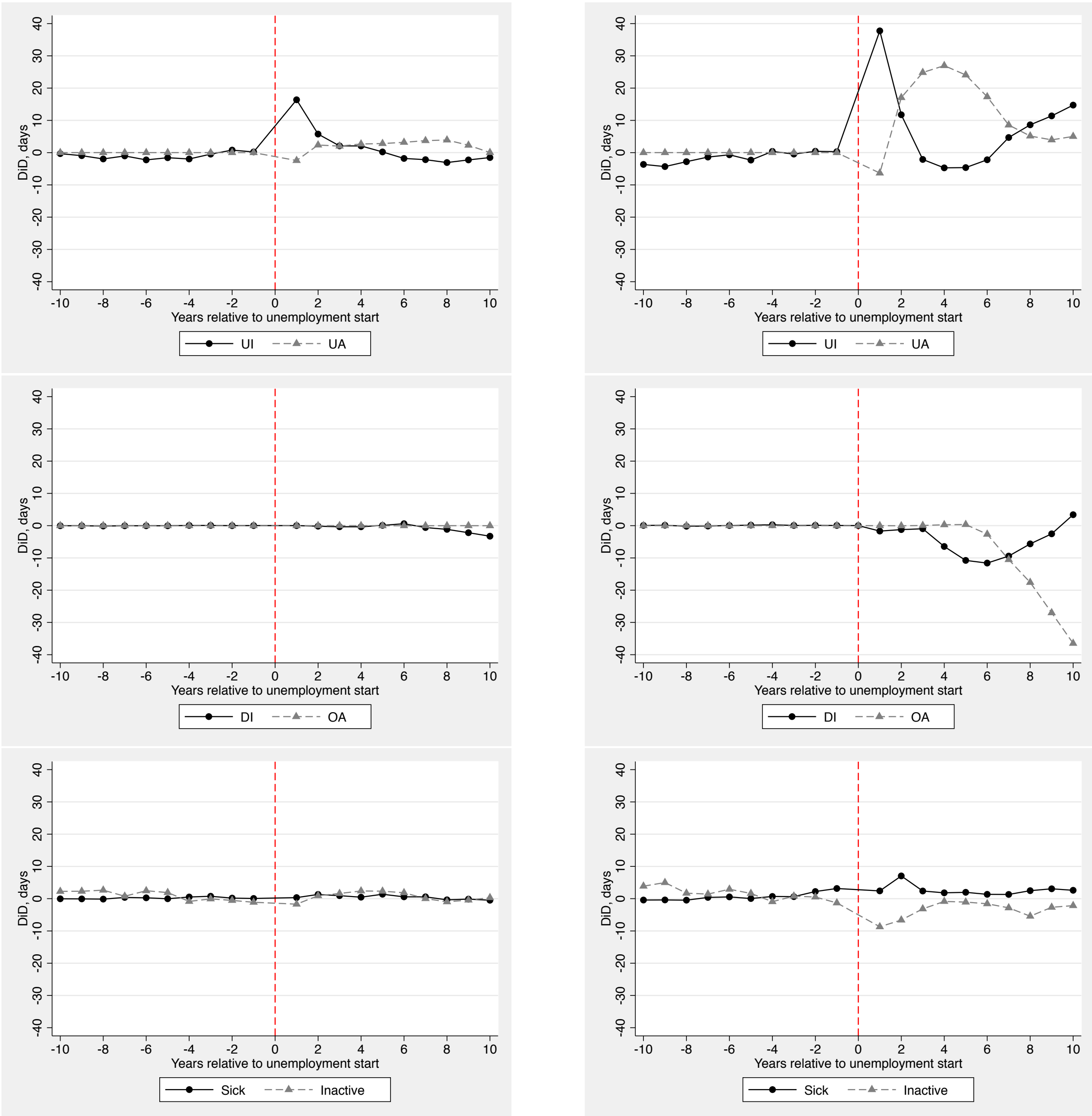
While the observed outcomes are unlikely to be the effect of a subsequent reform, it could still be some form of interaction with other programs inducing more permanent drop out of the labor force. In fact, there is substantial decline in labor force participation among older job seekers starting from year 5 after unemployment entry when all individuals of this treatment group have reached age 55. Employment levels up to year 5 after unemployment start are fairly stable allowing for a clean interpretation of the reform effect.[18] Thus, the estimated differences up to year 4 after unemployment entry are the result of the reform. Beyond that, it is more problematic to account the estimated differences to the reform alone because it is likely that they are at least partially driven by program substitution. Disentangling both the reform effect from eased access to other programs is not possible in the current setting.

[17]See appendix Figure 2.14

[18]See appendix Figure 2.15 for employment levels of older job seekers.

Figure 2.9: Treatment Effects by Age for Days on Different Social Benefits

(a) Younger **(b)** Older



Notes: Figure 2.9 shows treatment effects for each year before and after unemployment entry for days of benefit receipt by different type of benefits: UI: unemployment insurance, UA: unemployment assistance, DI: disability insurance, OA: old-age pension. Subfigure (a) is for the subsample of job seekers aged 45 or younger, Subfigure (b) is for the subsample of job seekers aged 50 or older. The full set of control variables is used in the regressions (see notes of Table 2.4) Corresponding regression results are available in Appendix Table 2.10. Own illustration using data from ASSD.

2.5 Conclusion

This paper analysis how an extension in PBD affects post-unemployment outcomes. The main outcome considered is earnings. To assess long-term effects of increases in PBD, I focus on earnings from the start of unemployment up to 10 years. With unemployment entry, earnings for the treatment group drop substantially compared to those of the control group. Earnings for treated individuals are lower beyond the period of initial unemployment because they remain longer nonemployed. As time goes more treated individuals find back to employment and the difference in earnings shrinks. However, also conditional on employment, treated individuals have lower earnings, suggesting that they get worse jobs. Jobs are worse as measured with both daily earnings and employment days. Daily earnings are permanently lower for treated individuals. That the difference in employment days shrinks over time suggests that the treatment group manages to improve – at least along one dimension of job quality. The extension in PBD has clear implications beyond unemployment itself but also beyond the first employment spell after unemployment. Focusing on the first job and the re-employment wage after UI overlooks important implications: adjustments both in employment and over time.

Extending PBD has a differential impact by age group. Younger job seekers incur a smaller earnings loss and virtually everyone returns to employment. However, increased PBD still has a substantial negative impact on post-unemployment earnings. Older job seekers incur a larger earnings loss, remain nonemployed longer and rely more heavily on unemployment insurance and assistance. While these results are true in the first four year after unemployment entry, the long-run effects for older job seekers are potentially partly driven by interactions with other programs. As results on programme interaction remain suggestive here, it would be interesting to study more thoroughly long-term implications of extended PBD with such interactions.

Acknowledgements

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2.6 Appendix

A Additional tables

Table 2.7: Sensitivity Analysis for Treatment Effects on Earnings

Year after UI start Outcome: Earnings	1	2	3	4	5	6	7	8	9	10
A. Baseline	-1,174.5***	-1,405.9***	-1,181.0***	-1,181.4***	-965.4***	-673.8***	-389.8***	-127.9	-11.4	184.0
	(94.2)	(121.2)	(129.0)	(135.1)	(140.2)	(144.0)	(146.6)	(148.1)	(149.1)	(149.2)
B. Only age 35-45	-821.7***	-854.5***	-657.3***	-723.0***	-655.9***	-530.0***	-412.8**	-148.0	-120.1	74.1
	(121.1)	(154.5)	(163.9)	(171.6)	(178.6)	(185.0)	(190.5)	(195.1)	(199.2)	(202.3)
C. High experience	-1,079.4***	-1,453.7***	-1,315.9***	-1,388.2***	-1,192.3***	-915.0***	-579.3***	-274.1*	-166.7	93.5
	(104.2)	(133.0)	(141.4)	(148.6)	(154.7)	(159.5)	(163.2)	(166.2)	(169.0)	(170.9)
D. Restricted years of inflow	-731.3***	-828.5***	-674.2***	-740.8***	-622.7***	-400.2**	-71.6	93.7	-62.5	83.0
	(133.1)	(171.1)	(181.0)	(188.0)	(193.5)	(197.5)	(200.6)	(203.6)	(205.8)	(206.3)
E. Only Austrians	-1,091.1***	-1,432.1***	-1,228.0***	-1,276.5***	-1,156.6***	-891.5***	-620.1***	-305.0*	-163.2	55.0
	(95.1)	(122.4)	(130.0)	(136.1)	(141.1)	(144.9)	(147.4)	(148.8)	(149.8)	(149.8)
F. No inflow 1 year before reform	-1,266.8***	-1,493.3***	-1,271.8***	-1,218.8***	-977.8***	-706.9***	-429.0***	-131.6	54.1	208.9
	(100.8)	(129.6)	(137.9)	(144.7)	(150.5)	(154.8)	(157.9)	(160.1)	(161.3)	(161.3)

Notes: Table 2.7 contains DiD estimates for various sample restrictions for single years after unemployment inflow. Panel A corresponds to estimates in Table 2.4. Panel B focuses on a sample where individuals are between 35 and 45 years old at inflow. Panel C focuses on a sample of individuals with experience of at least 9 out of the last 15 years and 6 out of the last 10 years (see Table 2.1 for the definition of the treatment). Panel D uses only inflow from the years 1987 to 1991 instead of 1985 to 1996 as in Panel A. Panel E uses only inflow of Austrians. Panel F excludes inflow from the 12 months just before the reform. *Mean Y* refers to the mean outcome in levels of the treatment group before the reform. The full set of controls is used (see notes of Table 2.9). Standard errors are in brackets and clustered on individual level. Significance is indicated as follows: * ($p<0.1$), ** ($p<0.05$), *** ($p<0.01$).

Table 2.8: Robustness: Median Earnings and Unobserved Heterogeneity

Year after UI start	1	2	3	4	5	6	7	8	9	10
A. Normalized earnings	-0.043***	-0.053***	-0.041***	-0.040***	-0.029***	-0.015**	-0.002	0.010	0.015**	0.024***
	(0.004)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
N	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728
B. Earnings										
R^2	0.146	0.153	0.154	0.157	0.161	0.166	0.172	0.178	0.183	0.188
δ_O	17.526	-6.853	-3.952	-3.417	-2.511	-1.611	-0.886	-0.261	-0.004	0.380
N	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728
C. Empl.										
R^2	0.156	0.141	0.139	0.143	0.150	0.158	0.171	0.183	0.195	0.205
δ_O	2.756	3.112	3.628	3.859	5.407	3.210	-44.891	11.207	6.197	5.223
N	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728
D. Earnings | empl.										
R^2	0.106	0.125	0.127	0.126	0.126	0.124	0.126	0.128	0.120	0.120
δ_O	-6.634	-2.163	-1.826	-1.807	-1.102	-1.194	-1.104	-0.641	-0.695	-0.555
N	104,485	111,162	107,901	103,545	99,033	94,052	88,616	83,099	77,594	72,103

Notes: Table 2.8 contains DiD estimates for earnings normalized with median earnings in age-experience groups in Panel A. Panels B, C and D show the test statistic (δ_O) for the influence of unobserved heterogeneity from Oster (2013). The test asks how much more important unobserved relative to observed heterogeneity has to be to reach a point estimate of zero for a desired level of $\tilde{R}^2$. Calculations are based on the assumption that the observed R^2 has to increase by 30%. A value of δ_O between 0 and 1 implies that already mild unobserved heterogeneity yields a point estimate of zero. A value larger than one means that unobserved heterogeneity has to be more important than observed heterogeneity. A negative value implies that unobserved heterogeneity would drag the point estimate in the other direction than observed heterogeneity does. The full set of controls is (see notes of Table 2.9). Standard errors are in brackets and clustered on individual level. Significance is indicated as follows: * ($p<0.1$), ** ($p<0.05$), *** ($p<0.01$).

Table 2.9: Decomposition of Treatment Effects on Earnings Over Time

Year after UI start	1	2	3	4	5	6	7	8	9	10
A. Earnings	-1,174.5***	-1,405.9***	-1,181.0***	-1,181.4***	-965.4***	-673.8***	-389.8***	-127.9	-11.4	184.0
	(94.2)	(121.2)	(129.0)	(135.1)	(140.2)	(144.0)	(146.6)	(148.1)	(149.1)	(149.2)
Mean Y	9,272	13,420	13,757	13,645	13,165	12,427	11,500	10,416	9,330	8,252
N	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728
B. Empl.	-0.060***	-0.039***	-0.031***	-0.024***	-0.022***	-0.005	0.008	0.015***	0.021***	0.032***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)
Mean Y	0.720	0.749	0.724	0.687	0.644	0.596	0.542	0.489	0.437	0.385
N	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728
C. Earnings | empl.	-0.087***	-0.066***	-0.059***	-0.062***	-0.039***	-0.040***	-0.040***	-0.025**	-0.025*	-0.020
	(0.012)	(0.011)	(0.010)	(0.011)	(0.011)	(0.011)	(0.012)	(0.012)	(0.013)	(0.014)
Mean Y	9.210	9.600	9.680	9.720	9.750	9.763	9.775	9.769	9.754	9.748
N	104,485	111,162	107,901	103,545	99,033	94,052	88,616	83,099	77,594	72,103

Notes: Table 2.9 contains DiD estimates for Earnings (Panel A), employment (Panel B) and conditional earnings (Panel C) split by year after unemployment entry. Column 1 contains results when the outcome is measured in the first year after unemployment entry. Column 2 contains results when the outcome is measured in the second year after unemployment entry and so forth. *Mean Y* refers to the mean outcome in levels of the treatment group before the reform. The full set of controls is used including the local unemployment rate, daily wage and tenure of the last job, experience in the last 5, 10 and 15 years, earnings and unemployment days in the 10 years before unemployment inflow, indicator variables for industry, region, year, month, age, family status and education, higher order polynomials of tenure, last daily wage and all experience measures. Significance is indicated as follows: * (p<0.1), ** (p<0.05), *** (p<0.01).

Table 2.10: Decomposition of Nonemployment

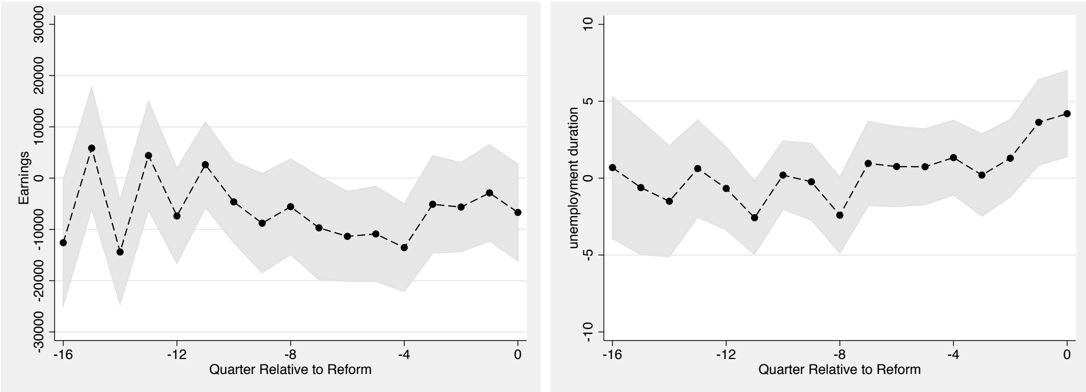
Year after UI start	1	2	3	4	5	6	7	8	9	10
A. Employment	-17.815***	-17.268***	-12.469***	-11.569***	-8.272***	-3.395*	1.572	4.494**	5.864***	8.498***
	(1.351)	(1.686)	(1.759)	(1.813)	(1.854)	(1.884)	(1.897)	(1.908)	(1.913)	(1.904)
Mean Y	152.14	215.25	215.63	208.94	196.69	181.69	165.21	148.22	131.79	115.95
N	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728
B. Unempl Insurance	23.741***	8.457***	1.543	0.203	-1.514	-2.345**	-0.531	0.565	2.618***	3.950***
	(1.127)	(1.187)	(1.139)	(1.107)	(1.113)	(1.116)	(1.105)	(1.050)	(0.964)	(0.849)
Mean Y	165.76	80.75	69.43	62.47	61.37	61.21	58.85	53.09	43.63	33.53
N	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728
C. Unempl Assistance	-3.632***	7.861***	9.848***	11.181***	11.191***	9.769***	8.004***	7.592***	6.504***	5.688***
	(0.172)	(0.430)	(0.482)	(0.537)	(0.574)	(0.606)	(0.642)	(0.714)	(0.812)	(0.914)
Mean Y	0.00	0.00	0.00	0.00	0.00	0.00	0.25	4.22	10.22	16.64
N	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728
D. Sickness	1.625***	2.933***	1.718***	1.239***	1.641***	0.737*	0.762*	1.289***	1.179***	1.223***
	(0.414)	(0.446)	(0.419)	(0.425)	(0.442)	(0.439)	(0.453)	(0.446)	(0.443)	(0.422)
Mean Y	15.67	13.39	11.91	11.89	11.51	11.70	11.48	10.75	10.20	8.75
N	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728
E. Disability	-0.551***	-0.400	-0.469	-1.868**	-3.025***	-3.268***	-5.678***	-7.913***	-9.933***	-12.085***
	(0.164)	(0.488)	(0.676)	(0.821)	(0.929)	(1.024)	(1.107)	(1.184)	(1.260)	(1.336)
Mean Y	2.65	13.44	24.37	35.57	46.32	56.30	66.81	77.24	87.64	98.14
N	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728
F. Old-age	-0.000	-0.015	0.009	0.060**	0.063**	-0.559***	-2.175***	-3.512***	-5.220***	-6.942***
	(0.000)	(0.013)	(0.017)	(0.025)	(0.030)	(0.134)	(0.288)	(0.402)	(0.493)	(0.576)
Mean Y	0.00	0.01	0.01	0.01	0.03	1.79	6.51	11.32	16.57	22.00
N	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728
G. Inactive	-3.369***	-1.561	-0.164	0.761	-0.084	-0.948	-1.964	-2.532	-1.033	-0.351
	(0.862)	(1.308)	(1.410)	(1.490)	(1.549)	(1.606)	(1.645)	(1.693)	(1.740)	(1.780)
Mean Y	29.01	42.42	43.90	46.37	49.30	52.56	56.14	60.41	65.19	70.24
N	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728	156,728

Notes: Table 2.10 contains DiD estimates for the change of days in different labor market states in a given year after unemployment entry. Days sum up to one year. *Inactive* is positive if no other state is found in the data and also includes death. *Mean Y* refers to the mean of the respective outcome for the treatment group before the reform. Full controls are used (see notes of Table 2.9). Standard errors are in brackets and clustered on individual level. Significance is indicated as follows: * (p<0.1), ** (p<0.05), *** (p<0.01).

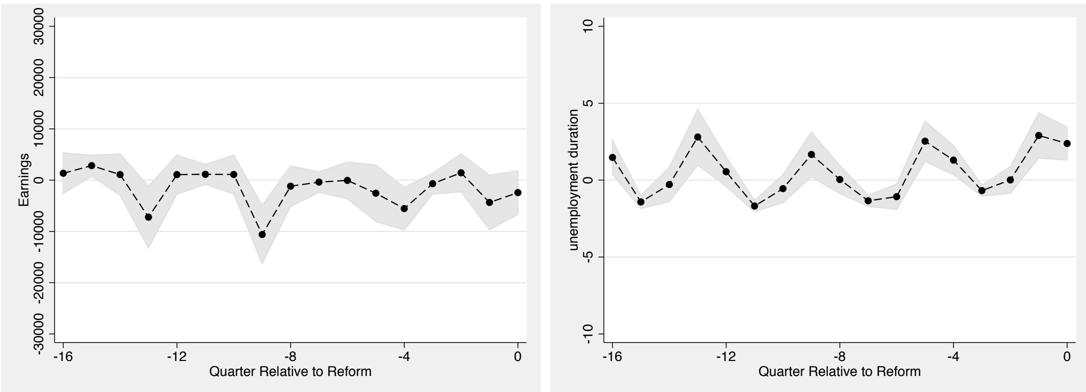
B Additional figures

Figure 2.10: Pre-trends for Women and Recalled Job Seekers

(a) Women

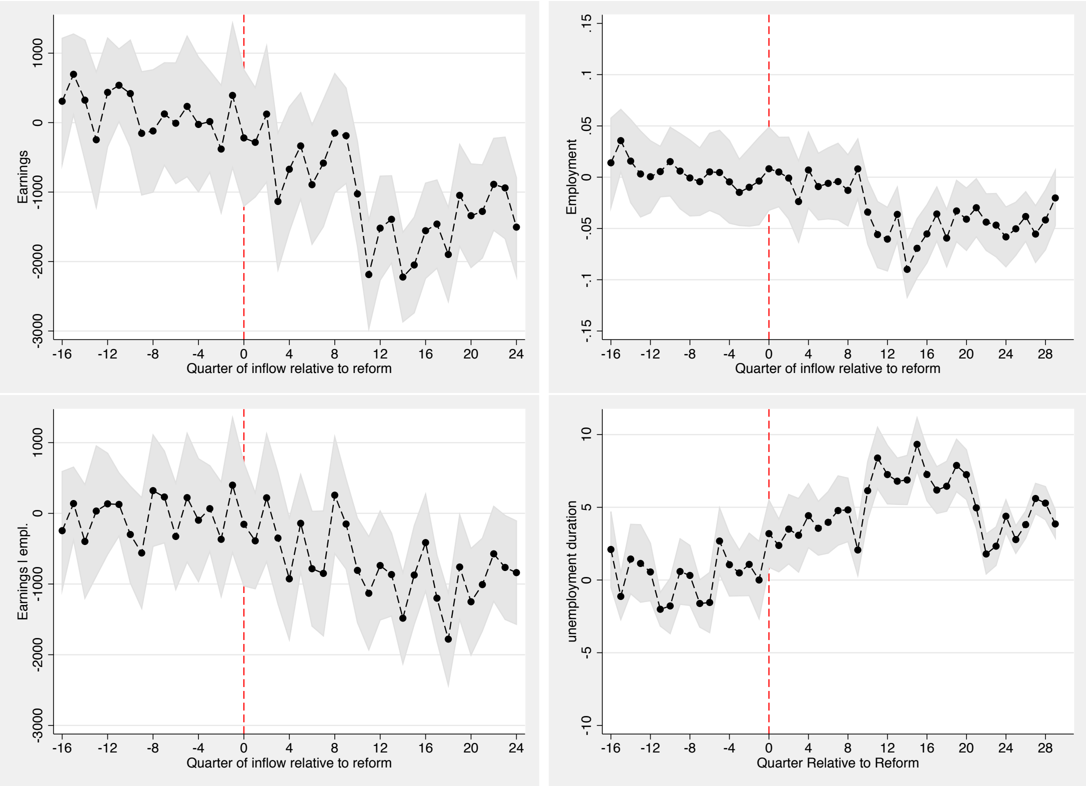


(b) Recalls



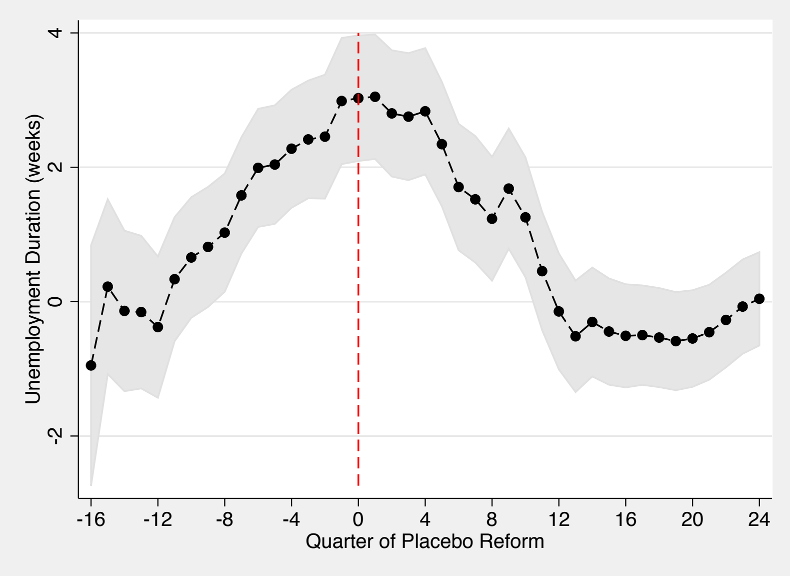
Notes: Figure 2.10 shows how treatment effects evolve in the 16 quarters before the reform for earnings (left) and unemployment duration (right). Dashed lines are treatment effects interacted with indicator variables for quarters relative to treatment. Shaded areas depict 95% confidence intervals. The sample in subfigure (a) includes women but no recalled job seekers. The sample in subfigure (b) includes women and recalled job seekers. Own illustration using data from ASSD.

Figure 2.11: Evolution of the Treatment Effect Around the Reform



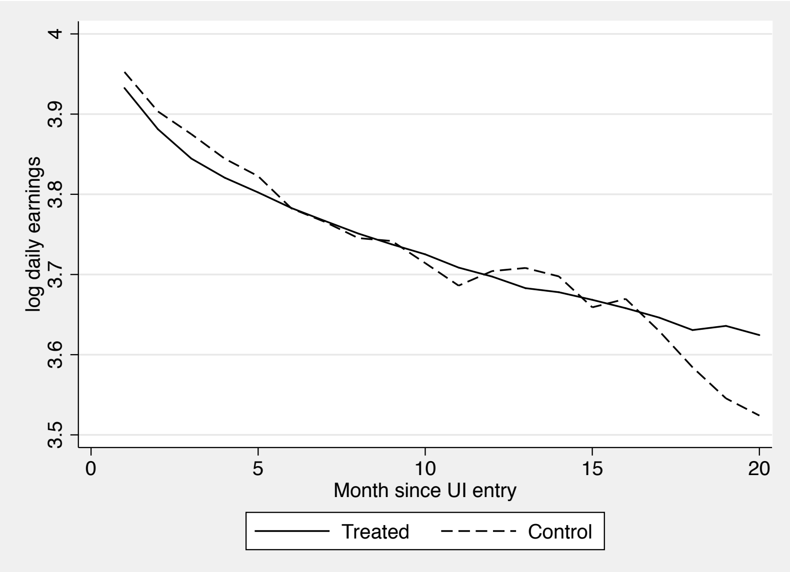
Notes: Figure 2.11 shows how treatment effects evolve in the quarters around the reform. Dashed lines are treatment effects interacted with indicator variables for quarter relative to treatment. Shaded areas depict 95% confidence intervals. Outcomes are measured in levels for the year after unemployment entry. The bottom right figure is for unemployment duration as outcome. Own illustration using data from ASSD.

Figure 2.12: Placebo Reforms for Initial Unemployment Duration



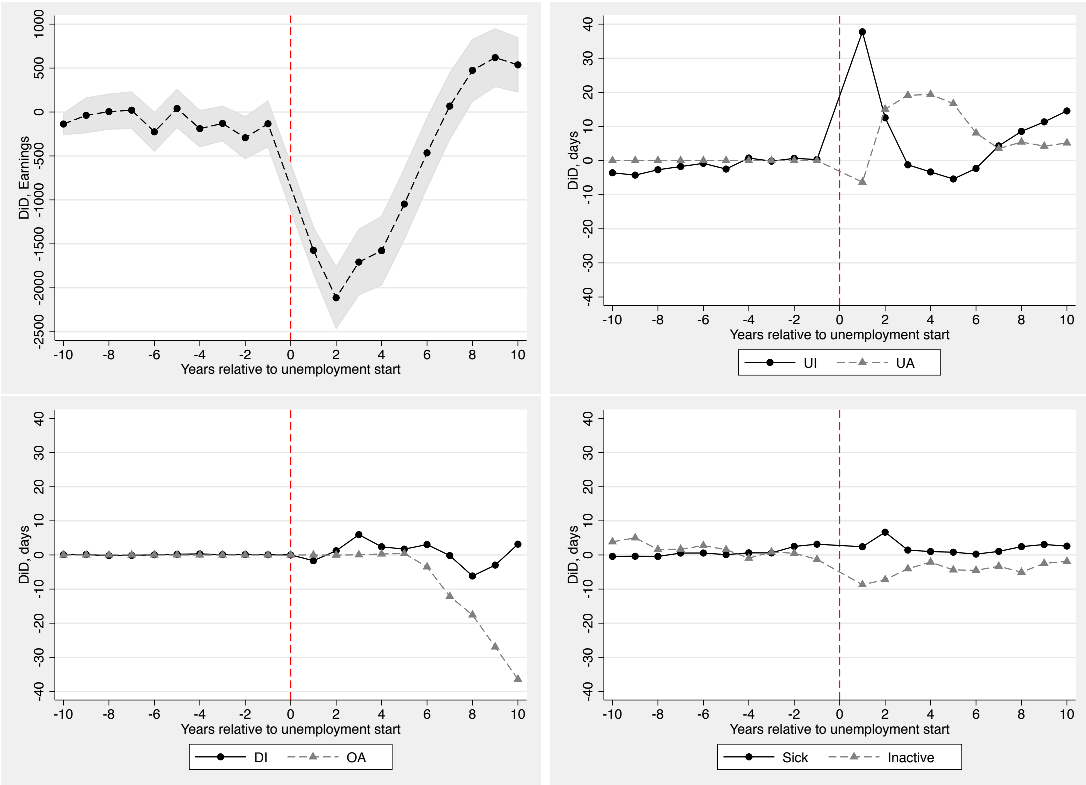
Notes: Figure 2.12 shows point estimates for unemployment duration from Differences-in-Differences regressions for a series of placebo reforms. Each placebo reform uses ± 3 years of inflow. Placebo reform dates start in August 1985 and shift every 90 days up to August 1996. The x-Axis measures quarters relative to the actual reform. Shaded areas depict 95% confidence intervals. The outcome is unemployment duration measured in weeks. Own illustration using data from ASSD.

Figure 2.13: The Effect of Extended PBD on Re-employment Wages Throughout the Non-employment Spell



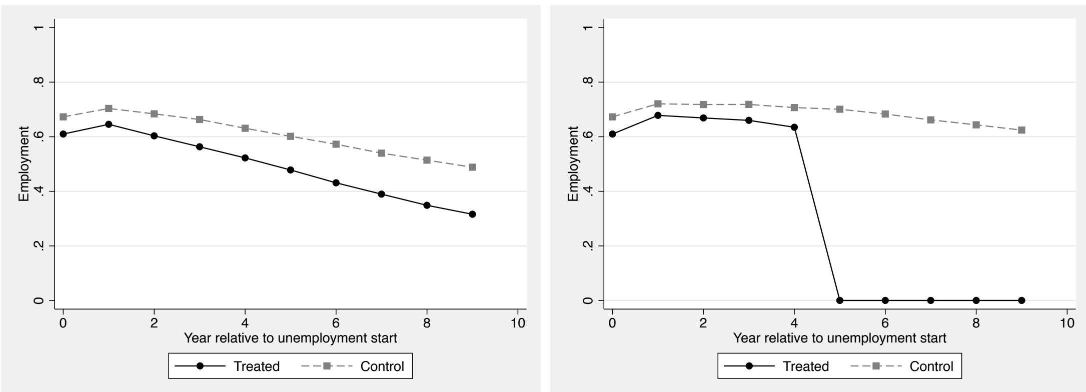
Notes: Figure 2.13 shows re-employment daily earnings paths for treatment and control group. The difference is estimated pointwise at each point of support using Differences-in-Differences estimation. The difference in point estimates is never statistically significant. The full set of control variables is used in the regressions (see notes of Table 2.4). Own illustration using data from ASSD.

Figure 2.14: Treatment Effects for Older Individuals Accounted for Subsequent Reforms



Notes: Figure 2.14 shows treatment effects for each year before and after unemployment entry for days of benefit receipt by different type of benefits: UI: unemployment insurance, UA: unemployment assistance, DI: disability insurance, OA: old-age pension. The full set of control variables is used in the regressions (see notes of Table 2.4), the regression also control variables for restricted access to disability and old-age due to reforms of the programs. See text for details. Own illustration using data from ASSD.

Figure 2.15: Employment Levels Older Job Seekers



Notes: Figure 2.15 shows employment levels for older job seekers separated by treatment and control group. The right graph additionally restricts the sample to outcomes where individuals are below age 55. After the sample is restricted to age below 55, employment becomes much more stable also in years 0 to 4. Own illustration using data from ASSD.

3 Spatial Search Strategies of Job Seekers and the Role of Unemployment Insurance

Joint with Elisa Guglielminetti, Rafael Lalive, and Etienne Wasmer

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3.1 Introduction

Most people do not work where they live, and travel times to work are substantial. Commuters travel about 70 minutes to and from work in the US, and about 60 minutes in Germany, the UK, and France (OECD (2010a)). Standard models of job search do not account for the fact that job seekers work outside their homes. Neglecting space is, perhaps, a useful simplification. But space has to matter in some decisions. The decision to accept a job will depend on commuting costs and mobility costs, not only the wage and its distribution. Job seekers who are looking for jobs will, optimally, want to use a reservation strategy involving both a reservation wage and a reservation commute distance, tied to each other. Further, even within acceptable commute distances, searching for jobs far away from one's residence may be expensive. Under liquidity constraints, job search efficiency may be seriously limited. This implies that unemployment insurance plays a role typically overlooked to improve the job search process.

The distance dimension of job search has several policy implications, beyond equilibrium unemployment, notably on the optimal design of unemployment compensation. Although explicit in many empirical and theoretical works, it is not central in most analyses. As a matter of fact, the commute time dimension is relevant in job acceptance decisions, and its impact is of the order of magnitude of the wage dimension; to illustrate, Table 3.1 shows that many job seekers report that the primary reason for rejecting a job offer is not for too low wages, but for too high distance. Excluding all reasons but wages and commute distance, the last column shows that 60% of job offers are rejected for too low wages, but 40% are rejected for too high commute distances. The commute distance is therefore a potentially first-order margin in job acceptance decisions. Of course, wage and distances interact: there might be a wage level making a commute distance acceptable.

Table 3.1: Reasons for Rejecting Offers

	%	**% excl. last 3**	**last 3 last 3 & hrs**	**% compared to wage rate**
1. rate of pay	12.1	21.8	24.7	59.7
2. temporary/insecure job	6.65	12.0	13.6	-
3. type of work	12.9	23.3	26.4	-
4. number of working hours	6.05	11.0	-	-
5. working time (day/night time, shifts...)	6.42	11.6	12.4	-
6. working conditions / environment	3.06	5.54	6.27	-
7. **distance to job / commuting**	**8.14**	**14.7**	**16.7**	**40.3**
8. could not start the job at required time	4.82	-	-	-
9. other reasons for not accepting	20.99	-	-	-
10. not yet decided	18.93	-	-	-
Sum	100	100	100	100

Source: Rupert et al. (2009).

In this paper, we explore these trade-offs and proceed as follows. We first derive a simple theory of job search in space that includes commute distance and optimal spatial search strategies. This will introduce the key concepts and discipline the empirical analysis in providing simple expressions for hazard rates. The three main endogenous variables are: the wage reservation strategy for a given commute distance (or equivalently the optimal reservation distance for a given wage); the optimal radius of job search in space; and within this range, the optimal intensity of search effort. We solve for the optimal acceptance decision where the interplay of accepted wages and accepted commute distance depends on the marginal rate of substitution between the two: individuals can buy short commutes with a lower wage or seek to be compensated with a higher wage for long commutes. This has obvious implications on job search strategies: indeed, once they correctly anticipate their future decision rules, unemployed individuals looking for a job may try to enter jobs that pay a higher wage and involve a shorter commute time relative to the previous job. We explore the implications for hazard rates and the role of unemployment insurance under various assumptions on liquidity constraints. As a matter of fact, in several countries, the spatial component of the costs of job search is either partly financed by the employment agencies, or deductible from income taxes[1].

We use an exhaustive panel of newly unemployed workers based on an administrative dataset in Austria, covering years 1995 to 2004 and overall more than 150 000 spells of unemployment to establish a few stylized facts related to commute distance and job acceptance decisions. The choice of Austria is motivated by data availability: we know the city of residence and the city of employment and can match these informations with information about transportation time from a private company which provided a matrix of travel time based on the existing network of roads and highways in 2000, approximately in the middle of our sample. The choice of Austria is also relevant because we want to isolate the commute time decision from the residential mobility decision. For unemployed individuals, we calculate that about 6% change their residency over the turn of non-employment. It turns out that the influence of mobility on the empirical results can be neglected in a first order, which considerably simplifies the analysis

In the data,we observe fairly high dispersion in the change of commuting distance and wage which make both margins relevant for unemployed individuals. We introduce an analysis of a competing risks model and its relative hazard ratios. Newly unemployed workers seem to start the job search from the same workplace as they used to be employed and looking for high wage jobs. As the unemployment spell gets longer, they tend to accept lower wages and progressively enlarge their range of search, ending up with a job farther away from their previous workplace. We offer evidence of a reservation frontier strategy in the wage/distance plane. We then investigate the role of policy and in particular unemployment insurance, in estimating Cox Proportional Hazard models. They provide measures of the causal effects of the unemployment insurance replacement rate, the social assistance replacement rate, and benefit duration (proxied by potential benefit duration) and show that their impact varies by destination (distance winners vs. losers, wage losers vs. wage winners).

The empirical analysis thus offers guidance in the solution and the calibration of an enriched model of the labor market capturing in a more accurate the regularities in the data. The model is therefore enriched along several dimensions. First, we allow for different unemployment compensation regimes: newly unemployed workers are covered by

[1]Eg. in the US, job search expenses are partly deductible from IRS. "*To qualify for a deduction, your expenses must be spent on a job search in your current occupation. You may not deduct expenses you incur while looking for a job in a new occupation; (...) ; If you travel to look for a new job in your present occupation, you may be able to deduct travel expenses to and from the area to which you travelled. You can only deduct the travel expenses if the trip is primarily to look for a new job ; (...) ; You cannot deduct job search expenses if you are looking for a job for the first time.*" Source: `http://www.irs.gov/uac/Job-Search-Expenses-Can-be-Tax-Deductible`. In France, a similar regime of tax deduction applies, complemented with direct subsidies of job search from Pôle Emploi (the employment agency): `http://vosdroits.service-public.fr/particuliers/F1640.xhtml`. In Austria, job search assistance covers parts of job search costs.

unemployment insurance, but they can subsequently loose it for a reduced level of benefits, in the unemployment assistance regime. We also allow individuals to target their job search activity in space, distinguishing effort inside and outside the previous workplace. Finally, we also introduce non-separability in consumption and search costs to allow for richer reservation strategies. Once calibrated, the model reproduces the empirical fact that, over time and as unemployment benefits decrease, the unemployed progressively adjust their reservation strategies: their reservation wage goes down and in addition they start prospecting in different areas. The model predicts that individuals remaining unemployed for longer time have a higher probability to enter less paying jobs and/or jobs located farther away from the previous job. The model delivers simple expression for all hazard rates (overall exit to employment, exits towards higher wages than in the previous job, exits towards lower wages, exit towards higher commute distances and towards lower distances) and all relative hazard rates.

A very large number of classical or more recent papers have been explicit about commute distance. Crampton (1999) has a discussion of the optimal location of vacancies and their number, illustrated by the classical papers by Seater (1979), Chirinko (1982) and more recently van Ommeren et al. (1997). Racial differences have been analyzed through the lens of distance and access to jobs in the spatial mismatch literature following Kain (1968): papers include Holzer (1986; 1987; 1988), Ihlanfeldt (1997), Zax and Kain (1996), Brueckner and Zenou (2003) and Coulson et al. (2001) and are summarized in Gobillon et al. (2007) and Zenou (2009); see also van Vuuren (mimeo) and Nenov (2015). The articulation between commuting decisions and mobility decisions has been studied by Rupert and Wasmer (2012) and applied to ethnic unemployment gaps in Gobillon et al. (2014) for commuting vs mobility decisions. More closely related to our work, the role of local labor markets has been investigated in Cheshire (1979), Rogerson (1982), Manning and Petrongolo (2011), Gobillon et al. (2011) and Marinescu and Rathelot (mimeo). The latter find in particular that job seekers's applications from a particular website, Carreer.Builder, decrease by 20% every 5 kilometres of distance between the applicant's address and the vacancy. Manning and Petrongolo (2011) also found a large decay, somewhat higher (approximately 80%), but for a different concept, the concept of job acceptance (and not of simple applications). Finally our work is connected to the large literature measuring the value of time across different transportation modes, at short and longer distances (see Brownstone and Small (2005) for road use and Hammadou and Jayet (2003) for longer transportation times). Recent papers, using experimental setups, have investigated the role of information on search strategies, including the broadness of search. See notably Altmann et al. (2015) and Belot et al. (2015).

Our paper also ties to a literature on the role of unemployment insurance for job finding and job quality. Ehrenberg and Oaxaca (1976) were the first to look at the effect of unemployment insurance on post-unemployment outcomes and find positive effects of unemployment benefits on post unemployment wages for different age groups and gender. Addison and Blackburn (2000b) provide evidence for a weakly positive effect of unemployment benefits on post unemployment wages. Centeno and Novo (2006) use a quantile regression approach to analyze the relationship between the unemployment insurance system and the quality of subsequent wages and tenure over the whole support of the wage and tenure distributions. They find a positive impact of unemployment benefits on each quantile of the wage and tenure distribution. Several recent studies, based on regression discontinuity designs, find little or no effects of Potential Benefit Duration (PBD), mostly looking at wage or job stability. Card et al. (2007a) and Lalive (2007) find little evidence on wages and/or job stability in the Austrian context. van Ours and Vodopivec (2008) find that a reduction in the potential benefit duration has only small effects on wages, on the duration of subsequent employment and on the probability of securing a permanent rather than a temporary job. Le Barbanchon (2012a) finds no effects on wages or employment. Two studies find positive effects of PBD on low wage earners or job seekers at risk of exhausting their benefits. Centeno and Novo (2009) detect a positive impact on the match quality for individuals at the bottom of the wage distribution. Caliendo et al. (2013) find that the unemployed who obtain a new job close to benefit exhaustion are more likely to leave subsequent employment and receive lower wages than than their counterparts with extended benefit duration. Two studies on Germany find negative effects of PBD extensions. Schmieder et al. (2012b) analyze the long-term effects of extensions in UI durations taking into account not only the initial, but also all recurrent non-employment spells. They find significant long-run effects of an extension in UI duration on the duration of non-employment up to three years after the start of the initial spell. Schmieder et al. (2016) study the effects of PBD changes on re-employment wages in Germany finding sharp negative effects of PBD extensions for older workers. Two studies on the Austrian context find positive effects of benefit extensions. Degen (2014) and Nekoei and Weber (2015) study the effects of PBD for job quality in Austria, exploiting a sharp increase in PBD from 30 to 39 weeks for workers aged 40 years or older. Both papers find a positive effect of prolonged PBD on wages on the order of 0.5 percentage points. Nekoei and Weber (2015) rationalize this finding in a directed job search framework and discuss the implications of this finding for policy.

Our paper extends and complements this rich literature in several respects. We build a simple theoretical search model where spatial decisions matter and make job acceptance depend on both wages and commute distance. Although several papers have done similar exercises, the model is flexible enough to provide functional forms that accurately match empirical concepts, such as hazards, sub-hazards and relative hazards ratios with respect to both commute distance and wage changes across jobs. We discuss whether and how much job seekers trade these two dimensions off. We use a rich framework and study how liqudity constraints may impede job search and how a subsidy might improve efficiency. The empirical exercise adds to the understanding of how unemployment benefits impact post-unemployment outcomes. This paper adds to this literature in assessing systematically not only how wages but also commuting distance is affected by the unemployment insurance system by estimating the effects on both outcomes simultaneously. This sheds light on how individuals not only decide for wages *or* distance but also for wages *and* distance. The estimation by means of the competing risk approach together with non-linearities in the determination of unemployment insurance parameters allows for a credible estimation of the impact of these parameters on both distance and wages. Overall, policy plays a crucial but complex role on job acceptance decisions and in turn on job search processes.

The paper is organized as follows. Section 3.2 introduces the key concepts behind the spatial analysis of job search. Section 3.3 provides various and hopefully exhaustive evidence of the role of space in job search and the spatial dispersion of commute distances, based on our rich data set of unemployment spells in Austria. In Section 3.4, we extend the model in order to provide a realistic calibration. In Section 3.5 we calibrate the model based on relative hazard ratios in the data and draw lessons for policy. Section 3.6 concludes.

3.2 A Simple Theory of Search in Space

The goal of this section is to provide the basic trade-offs of spatial search and commute and draw some implications of the theory. The model derives the reservation strategy defined here as *the minimum acceptable wage for a given commute distance. Commute distance* implies some costs and effort. Reciprocally, there is a maximum acceptable commute distance at a given wage. The agents, knowing their future strategy of job acceptance, optimally calculate the range of search, that is the maximum distance within which to prospect; finally, they determine the optimal intensity of search effort, captured by the arrival rate of job offers within the range of search.

3.2.1 Setup

Notations. Time is continuous. Individuals and firms discount the future at rate r. The level of benefits is b. Searching for a job is more costly in more remote areas. Let D be the radius of search, and $2\pi\lambda$ be the rate of arrival of search offers (where 2π is a simple proportionality factor coming from the integration of search in a circle around the individuals' location). Job seekers control both the intensity of search effort λ and the range of search at a cost $C(D, \lambda)$. At this stage we do not specify the nature of the search costs but they may be both pecuniary and non-pecuniary. We also assume perfect separability between search costs and consumption. Denote by $U(D, \lambda)$ the value of job search and by $W(w, \rho)$ the value of being employed at a wage w and at a commute distance ρ. The employed workers pay a commute cost $c(\tau\rho)$ which depends on commute time ρ and the cost of transportation τ. We also assume perfect separability in consumption and commute costs.

Unemployment and Employment Values. Each job offer consists in a random draw of wage and distance from a given two-dimensional distribution. We do not restrict the draws (w, ρ) to be independent. With notations $F_\rho(w)$ and $G(\rho)$ for the associated cumulated distributions of each variable separately, we can go one step further. In this case, the Bellman equations for job search are:

$$rU(D, \lambda) = b - C(D, \lambda) + 2\pi\lambda \int_0^D \left(\int_w Max[W(w, \rho) - U; 0] dF_\rho(w) \right) dG(\rho) \tag{3.1}$$

The value function for employment is:

$$rW(w, \rho) = w - c(\tau\rho) + s(U - W(w, \rho)) \tag{3.2}$$

3.2.2 Interior Solutions and Strategies

The surplus from employment can be easily calculated, given the linearity in income. Noticing that $\frac{\partial W}{\partial w}(w, \rho) = \frac{1}{r+s}$; and denoting by $R(\rho)$ the reservation wage associated with distance ρ, defined as $W(R(\rho), \rho) = U(D^*, \lambda^*) = U^*$, we can rewrite the value of employment as a linear function of w:

$$W(w, \rho) - U^* = \frac{w - R(\rho)}{r + s} = S(w, \rho) \tag{3.3}$$

where the notation $S(w, \rho)$ is the surplus value of holding a job paid w at a commute distance ρ.

We can now derive the reservation wage: it turns out to depend on commute costs and on the equity value of being unemployed under the optimal job search strategy. We have:

Lemma 1. *The reservation wage is linearly increasing in commute costs and in the unemployment value:*

$$R(\rho) = c(\tau\rho) + rU^*$$

It is convex or concave in the commute distance, depending on the convexity or concavity of commute costs. Convexity would result from disutility from time spent in commute, while concavity may result from optimization of transportation modes.

The interior optimal search strategies also follow immediately. Let $w^{\max}$ be the upper support of the wage distribution. Then, combining eq. 3.1 and 3.3 we have

$$rU(D,\lambda) = b - C(D,\lambda) + 2\pi\lambda \int_0^D \int_{R(\rho)}^{w^{\max}} S(w,\rho) dF_\rho(w) dG(\rho) \tag{3.4}$$

The first order condition on the radius is obtained by deriving eq. 3.4:

$$\begin{aligned} C'_D(D^*,\lambda^*) &= 2\pi\lambda \left(\int_{R(D^*)}^{w_{\max}} S(w,D^*) f_\rho(w) dw \right) g(D^*) \\ &= 2\pi\lambda g(D^*) \mathbb{E}_w S(w,D^*) \end{aligned} \tag{3.5}$$

so that $U(D,\lambda)$ is maximised with respect to the search strategy D when the marginal cost of searching at one more unit of distance is equal to the marginal gain. The marginal gain depends first on the direct impact on the flow of offers (first term of the right hand side) and second on the change of the surplus among acceptable offers (second term of the right hand side). The first order condition on optimal search effort affecting the arrival rate of offers λ reads as follows:

$$\begin{aligned} C'_\lambda(D^*,\lambda^*) &= 2\pi \int_0^D \int_{R(\rho)}^{w^{\max}} S(w,\rho) dF_\rho(w) dG(\rho) \\ &= 2\pi \mathbb{E}_{w,\rho} S(w,\rho) \end{aligned} \tag{3.6}$$

Both expressions show that the marginal cost has to equal the marginal gain of search, either with respect to extending the range of search by one marginal unit D, or by increasing the intensity of effort within the range. In both expressions, the marginal return on search involves the expected surplus value of holding a job.

Lemma 2. *Under separability of the cost function $C(D,\lambda)$, equation (3.5) implies that a higher arrival rate of offers λ is associated with a higher return on the range of search D, implying a complementarity of the two dimensions of search.*

Lemma 2 is not general, and under complementarity in the cost function $C(D, \lambda)$, the two search variables may be more substitute to each other: a higher λ raising the marginal cost of enlarging the range of search may in turn reduce the optimal radius D^*. The dominance of each mechanism is an empirical matter and we leave the question unanswered here.

3.2.3 Hazard Rates, Odds Ratios and Rejection Rate

The unemployment exit hazard is shaped by search intensity, search radius, and reservation wage as follows:

$$haz = 2\pi\lambda \left[\int_0^D \int_{R(\rho)}^{w^{\max}} dF_\rho(w) dG(\rho) \right]$$

The unemployment exit hazard depends on search intensity λ, search radius D, and on the reservation wage $R(\rho)$. Job seekers who search hard, or have a large search radius, or have a low reservation wage, will leave unemployment for a regular job faster. The unemployment exit hazard contains information on all three endogenous variables.

To anticipate the empirical part, we will decompose the total exit hazard into sub-hazard rates that reflect the quality of jobs the unemployed might find: paying better or worse, or being farther or closer to home than the previous job, like in a competing-risks framework. More precisely, w_{-1} is the wage, and d_{-1} is the commuting distance in the job prior to entering unemployment. w^+ refers to a wage increase, d^+ means an increase in commute distance relative to the previous job. Equivalently, w^- refers to a wage decrease, d^- means a decrease in commute distance relative to the previous job. The sub-hazard rate $haz(w^+, d^+)$, refers to job seekers accepting a new job with wage increase ($w > w_{-1}$) at the cost of commuting longer to this new job ($\rho > d_{-1}$). The sub-hazard of finding a better paying job located closer to home is defined as $haz(w^+, d^-)$, the sub-hazard of finding a worse paying job, located farther away from home is defined as $haz(w^-, d^+)$, and the sub-hazard rate of finding a worse paying job located closer to home is defined as $haz(w^-, d^-)$. We now express these sub-hazards in terms of the primitives of the model. Under the assumption that determinants of job search have not varied since the previous episode of job search, the search radius includes the previous distance, and the reservation wage is below the wage earned in the previous job : job seekers would accept the previous job if offered again to them. The four sub-hazard rates are then easy to write as:

$$sub - haz(w^+, d^+) = 2\pi\lambda \int_{d_{-1}}^{D} \int_{w_{-1}}^{w^{\max}} dF_\rho(w) dG(\rho) = 2\pi\lambda[1 - F(w_{-1})][G(D) - G(d_{-1})]$$
$$sub - haz(w^+, d^-) = 2\pi\lambda \int_{0}^{d_{-1}} \int_{w_{-1}}^{w^{\max}} dF_\rho(w) dG(\rho) = 2\pi\lambda[1 - F(w_{-1})]G(d_{-1})$$
$$sub - haz(w^-, d^+) = 2\pi\lambda \int_{d_{-1}}^{D} \int_{R(\rho)}^{w_{-1}} dF_\rho(w) dG(\rho) = 2\pi\lambda \int_{d_{-1}}^{D} [F_\rho(w_{-1}) - F(R(\rho))] dG(\rho)$$
$$sub - haz(w^-, d^-) = 2\pi\lambda \int_{0}^{d_{-1}} \int_{R(\rho)}^{w_{-1}} dF_\rho(w) dG(\rho) = 2\pi\lambda \int_{0}^{d_{-1}} [F_\rho(w_{-1}) - F_\rho(R(\rho))] dG(\rho)$$

As visible from the second equation, the first sub-hazard $sub - haz(w^+, d^-)$ does not depend on the endogenous variable D and on function R, which itself depends on the value of unemployment U; while the first one, $sub - haz(w^+, d^+)$, depends only on search radius, D, but not on the reservation wage. The last one, $sub - haz(w^-, d^-)$, depends on the reservation wage, $R(\rho)$, but not on the search radius. Finally, the third one, $sub - haz(w^-, d^+)$, depends on both search radius, and reservation wage. All sub-hazards depend on search intensity to the same extent, a result of our assumption that job seekers can not engage in directed search.

Under the simplifying assumption that F is not indexed by ρ in the expressions above, that is when the two distributions F and G are independent of each other, the relative hazards – the odds ratios – with respect to $sub - haz(w^+, d^-)$ can therefore be calculated as follows:

$$relhaz = \frac{sub - haz(w^+, d^+)}{sub - haz(w^+, d^-)} = \frac{[G(D) - G(d_{-1})]}{G(d_{-1})}$$
$$relhaz^2 = \frac{sub - haz(w^-, d^+)}{sub - haz(w^+, d^-)} = \frac{\int_{d_{-1}}^{D} [F(w_{-1}) - F(R(\rho))] dG(\rho)}{[1 - F(w_{-1})]G(d_{-1})}$$
$$relhaz^3 = \frac{sub - haz(w^-, d^-)}{sub - haz(w^+, d^-)} = \frac{\int_{0}^{d_{-1}} [F(w_{-1}) - F(R(\rho))] dG(\rho)}{[1 - F(w_{-1})]G(d_{-1})}$$

The first ratio of sub-hazards, $relhaz$, compares the chances of finding a better paying job farther away from home with the chances of finding a better paying job closer to home. This ratio should in principle depend on both the wage and distance in the previous job, but wage terms actually cancel each other out and the ratio only depends on the previous distance and the search radius. The last ratio $relhaz^3$ represents the relative probability of accepting a job with a wage cut compared to a better paying job, where both jobs are closer to home than the previous job. This odds ratio provides information on the reservation wage only, as both jobs are within the search radius. The ratio $relhaz^2$ represents the relative probability of accepting a job with a wage cut farther away from home relative to a better paying jobs closer to home. Since the "bad" jobs in the numerator are worse in both dimensions, the ratio $relhaz^2$ reflects the joint evolution of reservation wages and search radius. Odds ratios do not contain search intensity since it affects all sub-hazards to the same extent.

The job rejection rate is, in the general case:

$$reject = \int_0^D F_\rho(R(\rho))dG(\rho)$$

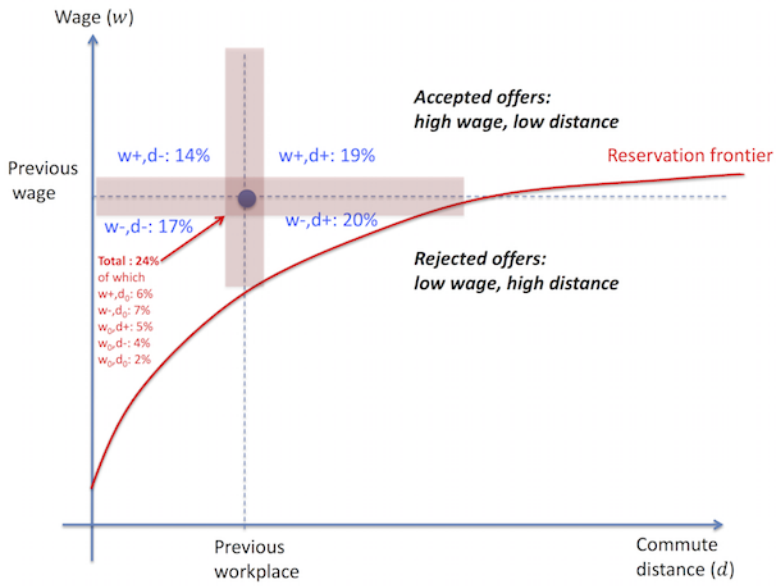
Under the assumption of independence of the joint distribution of wages and distance, the rejection rate increases in D: at a higher distance, it is more likely that the drawn wage will not compensate for distance.

3.2.4 The Effect of Distance on Wages

The model thus explicitly accounts for the role of distance on reservation wage and on expected, accepted wage. The reservation frontier in wage and distance can be represented as in Figure 3.1, here under the assumption of concave costs of distance $c(\tau\rho)$. The figure also displays the proportions of each unemployment-employment trajectory from the data used in next Section.[2]

[2]We use here the same notations as in previous sub-Section, as well as new notations for "wage stayers" (w_0) and "city stayers" (d_0). Labels $d+$, d_0 and $d-$ therefore reflect the trajectories towards longer, identical and shorter commuting distances respectively, while $w+$, w_0 and $w-$ represent trajectories towards jobs paid more than $+4\%$ than the previous job, similar wages, that is in the interval ($+4\%$;-4%) and finally paid less than 4% than the previous job.

Figure 3.1: Reservation Frontier and Acceptance-Rejection Areas.



Notes: Percentages reported on the Figure refer to the fraction in Austria of the newly employed individuals in each of the quadrants defined by the wage/commute distance in their previous job. Source: author's calculations from Section 3.

In the data, we do not directly observe the reservation distance but only accepted wages and accepted commute distances. When the two distributions in wages and distances are independent, it is possible to calculate conditional wages and their slope with respect to commute distance with a simpler formula. In this specific case we have:

$$w^e(\rho) = \frac{1}{1 - F(R(\rho))} \int_{R(\rho)}^{w^{\max}} wdF(w)$$

and the slope of w^e with respect to ρ is

$$\begin{aligned}
\frac{\partial w^e}{\partial \rho} &= \frac{c'(\tau\rho).f(R(\rho))}{[1 - F(R(\rho))]^2} \int_{R(\rho)}^{w^{\max}} wdF(w) + \frac{-c'(\tau\rho)R(\rho)f(R(\rho))}{1 - F(R(\rho))} \\
&= \frac{f(R(\rho))c'(\tau\rho)}{1 - F(R(\rho))} \left(w^e - R(\rho)\right)
\end{aligned}$$

The slope is clearly positive, as accepted wages are above the reservation one at any given distance. It is not linear and might be either convex or concave, depending on the features of the wage distribution $F(\cdot)$.

3.3 Empirical Analysis

3.3.1 Geography and Institutional Background

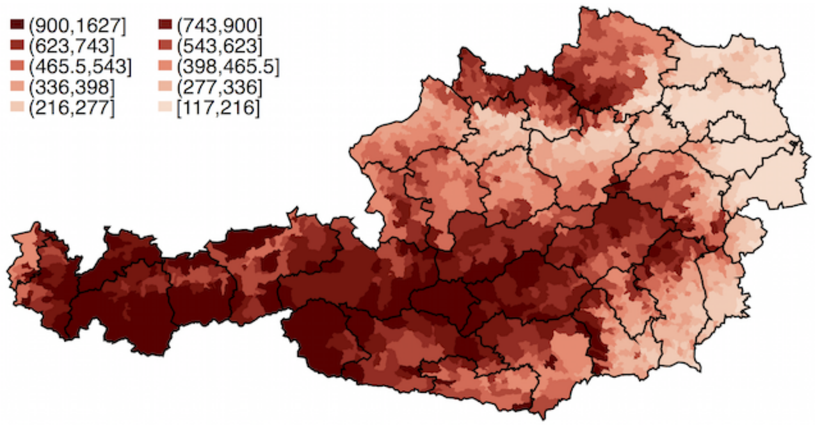
Time and costs associated to commuting are relevant for the majority of Austrian workers. Indeed, in the year 2001, 92% of the total workforce commuted and 86% of the total workforce commuted daily. 67% of the daily commuter cover the major commuting distance by car, 20% commute by public transport and 13% either walk or commute by bicycle. 68% of the daily commuting individuals work in a different municipality than they live in. Yet, 80% stay within a political region (there are 99 political regions), hence many stay in the same county, which means that mobility is limited in Austria. As people do not incur long commutes on average, one concern for our analysis might be that individuals try to avoid commuting by relocating. Although there can be benefits in terms of commuting, there is certainly a cost involved in relocating. Compared to the US, residential mobility in Austria is low. Fischer (2002) provides calculations for the US. For Austria, we calculate that less than 6% (between 10-15% for the US) change the residential municipality and less than 1.6% (above 5% for the US) cross the county border annually. In particular in our sample, less than 5% change the residence over the turn of unemployment[3].

The geography of Austria adds to make it an interesting country to study commuting. Austria is a relatively small country yet with potentially large commute distances due to the presence of the Alps and the particular longitudinal shape: the maximum distance from west to east is around 700 kilometres. Cutting through Munich in Germany, the distance between the northwestern city of Bregenz to Wien (Vienna) is 618 kilometres and six hours drive. The distance between the southern city of Klagenfurt to the northern city of Linz is only 251km but it takes 3 hours to reach the other city given the mountains. Figure 3.2 plots Austria and the altitude of each municipality. The white lines constitute borders of municipalities. The black lines depict the borders of NUTS3 regions. A dark colour indicates that the municipality is high above sea level. Altitude ranges from 110 to 1600 meters above sea level. The Alps in the middle of the country are clearly visible as are the flat parts in the east towards Hungary. This variety in the terrain is likely to have an impact on how individuals commute.

We will study the effects of unemployment insurance extensively. The unemployment system in Austria, as in many other countries, consists of a first part where eligible individuals receive Unemployment Insurance (UI) benefit (UB). The level of UI benefits is calculated based on base earnings, where base earnings refer to average earnings in the baseline period. The baseline period is the year $t-1$ for job seekers who enter

[3]Sources: CPS 2001 Statistik Austria, own calculations from tax records.

Figure 3.2: Altitude of Municipalities



Notes: The figure shows the altitude of municipalities measured at the town hall of each municipality. Source: Bundesamt fuer Eich- und Vermessungswesen.

unemployment between July and December of year t. The baseline period is the year $t-2$ for job seekers who enter unemployment from January to June in year t. Baseline earnings are multiplied with the replacement rate to calculate unemployment benefits. Benefits are capped from below and above, the cap being adjusted annually for inflation. We will exploit these caps to identify the effects of unemployment benefits in our analysis below.

The potential duration of unemployment benefits (PBD) is a function of past work experience and age. For instance, job seekers who have been working for a at least 3 out of the previous 5 years, and are 40 years or older when registering for unemployment benefits receive 39 weeks of unemployment benefits compared to 30 weeks if they are less than 40 years old.[4] A similar discontinuity exists at age 50, where PBD increases from 39 to 52 weeks, for job seekers who worked 9 out of the previous 15 years.

Once unemployment benefits are exhausted, individuals are eligible for means tested Unemployment Assistance (UA; Notstandshilfe) benefits. The means test includes in particular family income and wealth which makes it unlikely for many individuals to actually get UA benefits. Conditional on getting UA benefits they can be fairly high, as much as 92% of UB. UA does not end, but job seekers need to re-apply for UA once every 26 weeks.

[4]See Nekoei and Weber (2015) who analyze this discontinuity.

3.3.2 Data and Sample

We combine data from different sources to reach our final data set. First, the Austrian Social Security Database (ASSD)[5] contains detailed information on the work history for all private sector workers from 1972 to present. It contains both a unique plant and person identifier. Second, the unemployment register contains detailed information on both UI and UA benefits for the years 1988 to 2007. Third, we use data from a road trip planning firm to measure travelling time between any two municipalities.[6]

To construct our data set we obtain all unemployment spells from the ASSD that last at least for 7 days. For a given unemployment spell we figure out information about the last and next (if there is one) employment spell. For the relevant employer-employee relation before and after unemployment, we obtain the following variables: exact date of termination and start of the relation, average daily wage (yearly contribution to the social security system divided by the number of working days), geographic location (municipality-level[7]), industry affiliation of the employer. For the individual we know the month of birth and gender and we can calculate tenure on either job, experience, sickness, occupation (blue/white collar).

The two variables age and experience allow us to calculate the potential benefit duration for UI benefits. Knowing this duration, we are able to distinguish between time of UI and (potential) UA receipt for each unemployment spell. For each unemployment spell we know the exact duration on days. Furthermore, the data allow us to calculate the non-employment duration. This is the number of days between the succeeding and the previous job. The ASSD data allow us to determine the basis on which benefit are calculated, which is typically different from the previous wage. We can identify the unemployment spells from the ASSD data in the unemployment register. From the unemployment register, we obtain the municipality of residence, the UI and UA benefit level, education and information on the family situation.

The third data set, road trip planning data from the year 2000, contains time and distance in kilometres between any pair of municipalities. This distance is measured between the centroids of the municipalities. Hence, for each unemployed individual we

[5]See Zweimüller et al. (2009) for a detailed description of the data set.

[6]Our data set only contains individuals who live and work in Austria. Hence we do miss commuters across national borders. Official statistics suggest that we do not miss out many cases. From the census 2001, there are 3.6 millions individuals listed as employed of which 57,730 (1.59%) said they live in Austria but work abroad, mostly in Germany. We know the precise number of Austrian cross border workers only for Switzerland. Namely in 2013Q3 there were 8,119 Austrians who crossed the border at least once a week to work in Switzerland. Back in 2002Q3 the figure was 6,985. Conversely, the tax data authority indicates that of those who have to pay taxes in Austria, 5.8% live abroad and this latter number also includes individuals temporarily living abroad.

[7]There were 2376 municipalities in 2014.

can calculate previous and succeeding distance to the workplace[8].

We restrict the analysis along some dimensions. First, we focus on unemployment spells starting between January 1995 and December 2004. The main reason to start after 1994 is to avoid interactions with a major change in the unemployment system that extended the potential benefit duration substantially for certain individuals[9]. Second, we include individuals aged 20 to 54 at the start of unemployment. We do not want to include older individuals to avoid interactions between unemployment and early retirement, which is strong in Austria as assessed in Inderbitzin et al. (2016). Third, we exclude individuals with a commute of more than two hours prior to unemployment. These are most likely weekly commuters and may have a different search patterns relative to daily commuters, who are of main interest in our study. Fourth, individuals who quit voluntarily[10] and those who return to the same employer are excluded. The particular data we use need two more restrictions. First, the average daily wage we are measuring confounds hours and the wage rate. This is a major problem for women but not for men. We focus on men because virtually all men work full time. Second, the commuting time we measure is not door to door but municipality to municipality. This is a potential source of measurement error which may be particularly relevant in metropolitan areas, where the actual commuting time is highly affected by the exact location of residences and workplaces. As a robustness check, we exclude the largest 5 cities in Austria except Vienna, namely Graz, Linz, Salzburg, Innsbruck and Klagenfurt. For Vienna we can identify the 23 districts and treat each of them as single municipalities. This is not possible for the other cities.

A first look at the structure of commuting in Austria is given in Figures 3.3 and 3.4. Figure 3.3 illustrates commuting time by place of residence. It is evident that individuals who live in mountainous areas commute longer. Those who live in flatter areas (north east) or valleys (west) experience shorter commutes. Hence, workers do trade-off distances with amenities (e.g. living in the countryside). If we draw the same picture not by municipality of residence but municipality of work (Figure 3.4), we do not see such a clear geographical pattern: for each workplace, there is a more balanced distribution of commute time and we do not find strong evidence of concentration in space of larger commute times by workplace.

[8]Note that our data contains information on plant location. People who work in headquarters of firms are not in our data as their municipality code is missing.

[9]See Lalive and Zweimueller (2004) for an analysis of this reform.

[10]Identified through a waiting period of 28 days.

Figure 3.3: Average Commuting Time by Residency

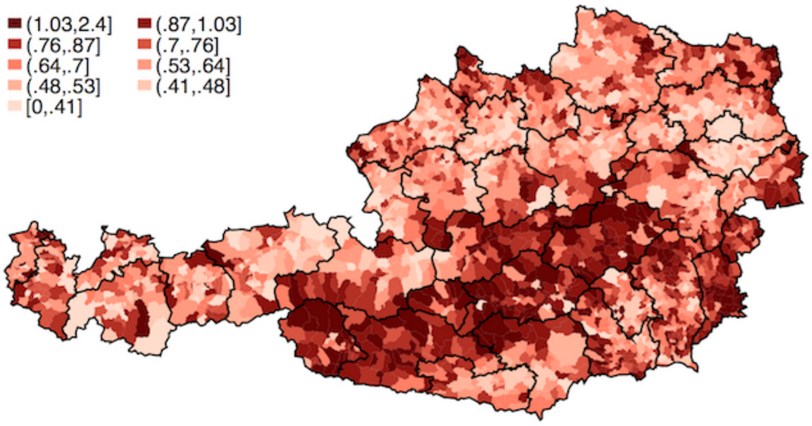
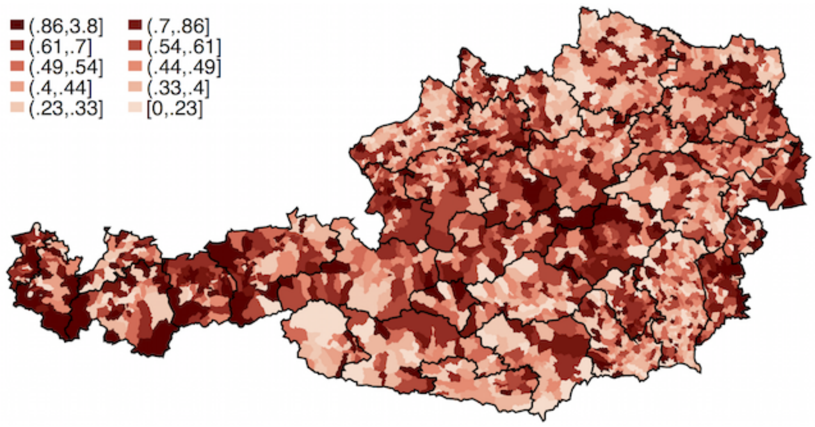


Figure 3.4: Average Commuting Time by Workplace



3.3.3 Stylized Facts on Wage and Commute Changes

We report in Table 3.2 the summary statistics for the full sample (154,677 spells). We also split these statistics for each of the four possible outcomes (where $w+$, $w-$, $d+$, $d-$ represent, respectively, workers experiencing a transition from a lower to a higher paid job ($w+$), workers experiencing a transition from a higher to a lower paid job ($w-$), workers experiencing a transition from a closer job to a job further away ($d+$), and finally workers experiencing a transition from a job further away to a closer job ($d-$). The latter subset also includes workers who find a job at the same distance, denoted hereafter by d_0: conditional on changes, there is a 16% mass of people remaining in the same city before and after a transition through unemployment.

Workers, on average, spend 25 weeks in non-employment; those who find a wage at least as high as the last wage spend 20 to 21 weeks in non-employment. Individuals finding a job at the same distance as the previous job are non-employed on average for 22 weeks. Workers finding a job at a different location are non-employed on average for a longer time (about 24 weeks). The number of weeks in registered unemployment is smaller (row 2), around 15 to 20 weeks. We also calculate potential benefit duration, which is around 32 weeks (row 3). The average replacement rate is around 40% for unemployment benefits in the unemployment regime (UI, row 4). Data also include information on the amount under an assistance regime (UA), which we will introduce in the next Section to enrich the model. Row 5 gives the mean replacement rate including zeros (that is, for workers eligible to the regular unemployment insurance regime) and row 6 gives the mean replacement rate for workers under the UA regime. The replacement rate of the UA regime is close to the UI regime. Indeed, once UA is granted, it amounts to around 90% of UI benefits which translates into the lower replacement rate despite the fact that the sample is much different - UI is populated by higher wage workers.

Previous daily wage is 59.98 euros (full sample); the next wage is 57.67 after exiting non-employment. For those getting a higher wage, the new wage is 67; for wage losers, instead, the mean wage is around 50 euros. Previous commute time is .443 of an hour (that is 0.438x60=26.58 minutes one way). Commute time after is 0.62 of an hour, almost 40 min. On average those who commute more now commute around an hour; those who commute less commute 0.298 of an hour, that is 18 minutes.

Table 3.2: Summary Statistics

	Full Sample Mean (Std.Dev.)	$w+$ Mean (Std.Dev.)	w_0 Mean (Std.Dev.)	$w-$ Mean (Std.Dev.)	$d+$ Mean (Std.Dev.)	d_0 Mean (Std.Dev.)	$d-$ Mean (Std.Dev.)
Non-empl (wks)	25.04 (37.88)	20.35 (29.83)	20.84 (28.75)	27.68 (38.62)	24.53 (35.67)	21.95 (31.36)	23.78 (33.72)
Unempl (wks)	18.04 (23.62)	15.22 (19.74)	16.42 (20.44)	20.29 (26.08)	17.96 (23.43)	16.61 (21.42)	17.94 (23.44)
PBD [weeks]	32.35 (6.23)	31.91 (6.01)	32.63 (6.4)	32.61 (6.34)	32.2 (6.08)	32.59 (6.48)	32.35 (6.27)
RR UI benefits (B)	0.4 (0.156)	0.442 (0.177)	0.396 (0.137)	0.365 (0.129)	0.402 (0.155)	0.401 (0.157)	0.399 (0.156)
RR UA (overall)	0.032 (0.111)	0.03 (0.116)	0.027 (0.101)	0.034 (0.106)	0.032 (0.11)	0.028 (0.104)	0.033 (0.112)
RR UA (eligible) (b)	0.374 (0.126)	0.432 (0.138)	0.379 (0.104)	0.338 (0.105)	0.375 (0.124)	0.378 (0.123)	0.373 (0.127)
UI benefits	24.45 (5.42)	23.41 (5.34)	24.37 (5.12)	25.27 (5.3)	24.41 (5.33)	23.96 (5.33)	24.58 (5.41)
UA benefits	20.56 (6)	19.53 (5.79)	20.63 (5.56)	21.12 (5.99)	20.57 (5.84)	19.89 (5.72)	20.69 (6.09)
Altitude [100m]	4.262 (2.488)	4.332 (2.525)	4.273 (2.428)	4.158 (2.431)	4.2 (2.425)	5.071 (2.619)	3.921 (2.372)
Time to Next Large City	24.58 (25.41)	25.17 (25.64)	25.3 (25.43)	23.49 (24.98)	24.94 (25.24)	26.27 (27.24)	22.88 (24.42)
Wage Before ([Euros],w_{-1})	59.98 (21.44)	50.79 (14.99)	59.28 (17.02)	67.84 (23.51)	59.66 (21.77)	58.49 (19.85)	60.43 (21)
Wage After (w^+, w^-)	57.67 (19.39)	66.67 (19.38)	59.24 (17.03)	49.24 (15.97)	58.37 (19.47)	56.45 (19.02)	57.33 (19.42)
Change Wage	-2.079 (22.8)	15.872 (13.736)	-0.04 (1.399)	-18.598 (19.707)	-1.293 (23.71)	-2.038 (20.887)	-3.096 (22.421)
Commuting before ([hrs], d_{-1})	0.443 (0.412)	0.441 (0.412)	0.438 (0.407)	0.448 (0.413)	0.323 (0.338)	0.273 (0.373)	0.675 (0.41)
Commuting after ([hrs], d^+, d^-)	0.615 (0.789)	0.647 (0.822)	0.601 (0.77)	0.59 (0.762)	0.989 (0.977)	0.273 (0.373)	0.298 (0.303)
Change Commuting	0.171 (0.807)	0.206 (0.838)	0.163 (0.778)	0.143 (0.786)	0.666 (0.902)	0 (0)	-0.377 (0.349)
Numb. Children	1.111 (0.796)	1.095 (0.767)	1.119 (0.795)	1.118 (0.814)	1.109 (0.791)	1.099 (0.756)	1.113 (0.81)
Married	0.401 (0.49)	0.377 (0.485)	0.416 (0.493)	0.416 (0.493)	0.395 (0.489)	0.392 (0.488)	0.41 (0.492)
White Collar	0.166 (0.372)	0.16 (0.366)	0.153 (0.36)	0.158 (0.364)	0.144 (0.351)	0.166 (0.372)	0.172 (0.377)
Exp 0-1.99y [Y]	1.7 (0.333)	1.668 (0.339)	1.691 (0.33)	1.726 (0.327)	1.702 (0.333)	1.696 (0.328)	1.693 (0.335)
Exp 2-4.99y [Y]	2.494 (0.583)	2.455 (0.604)	2.495 (0.564)	2.521 (0.571)	2.494 (0.584)	2.489 (0.582)	2.487 (0.587)
Exp 5-9.99y [Y]	3.438 (1.584)	3.278 (1.645)	3.473 (1.553)	3.519 (1.548)	3.38 (1.608)	3.457 (1.578)	3.437 (1.582)
Sector 1	0.026 (0.16)	0.029 (0.168)	0.032 (0.177)	0.022 (0.148)	0.024 (0.152)	0.041 (0.199)	0.023 (0.149)
Sector 2	0.701 (0.458)	0.677 (0.468)	0.7 (0.458)	0.723 (0.448)	0.717 (0.451)	0.674 (0.469)	0.693 (0.461)
Sector 3	0.211 (0.408)	0.236 (0.425)	0.207 (0.405)	0.189 (0.392)	0.194 (0.395)	0.23 (0.421)	0.224 (0.417)
Count	154677	61204	17852	68713	68744	24854	54171
Years	1995-2004						

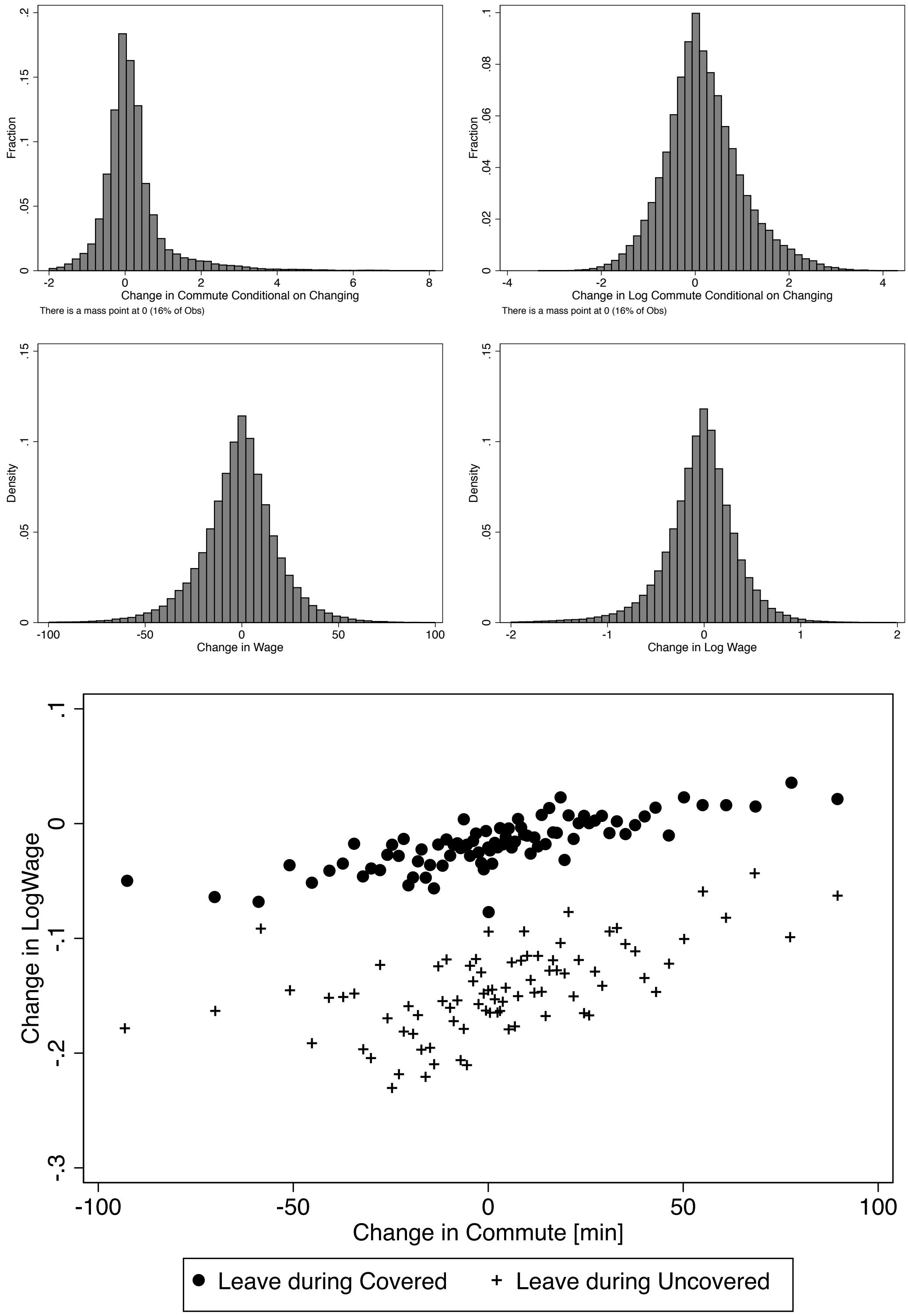
Notes: The Table reports summary statistics from the analysis sample split by exit. Women are excluded. See text for the definition of exits. Source: ASSD, own calculations.

In Figure 3.5 we take a closer look at the distribution of commute and wage changes between any two jobs spaced out by an unemployment spell. The first and the second rows represent the distribution of commute and wage changes, respectively, in levels (left panel) and in logs (right panel). The dispersion is quite large; in relative terms, given that the mean commute time is about 30 minutes, it turns out that the typical dispersion is higher for commute distances. From the left panels we can notice that commuting times are right skewed, while wages are symmetric.

The third row of Figure 3.5 also reports in the scatter plot of changes in log wages and commuting distance changes per unemployment status: we distinguish between individuals who find a new job while they are receiving unemployment benefits (black circles) and individuals who find a job only after they have exhausted unemployment benefits and eventually receive unemployment assistance benefits (crosses). In both cases, the correlation appears to be positive: higher changes in commute time are associated with larger wage gains while lower commute distances are typically associated with negative wage growth between the previous and the next job. This scatter plot is first evidence that time until a job is found matters: those finding a job under the UA regime face a lower net wage growth conditional on distance change or vice versa.

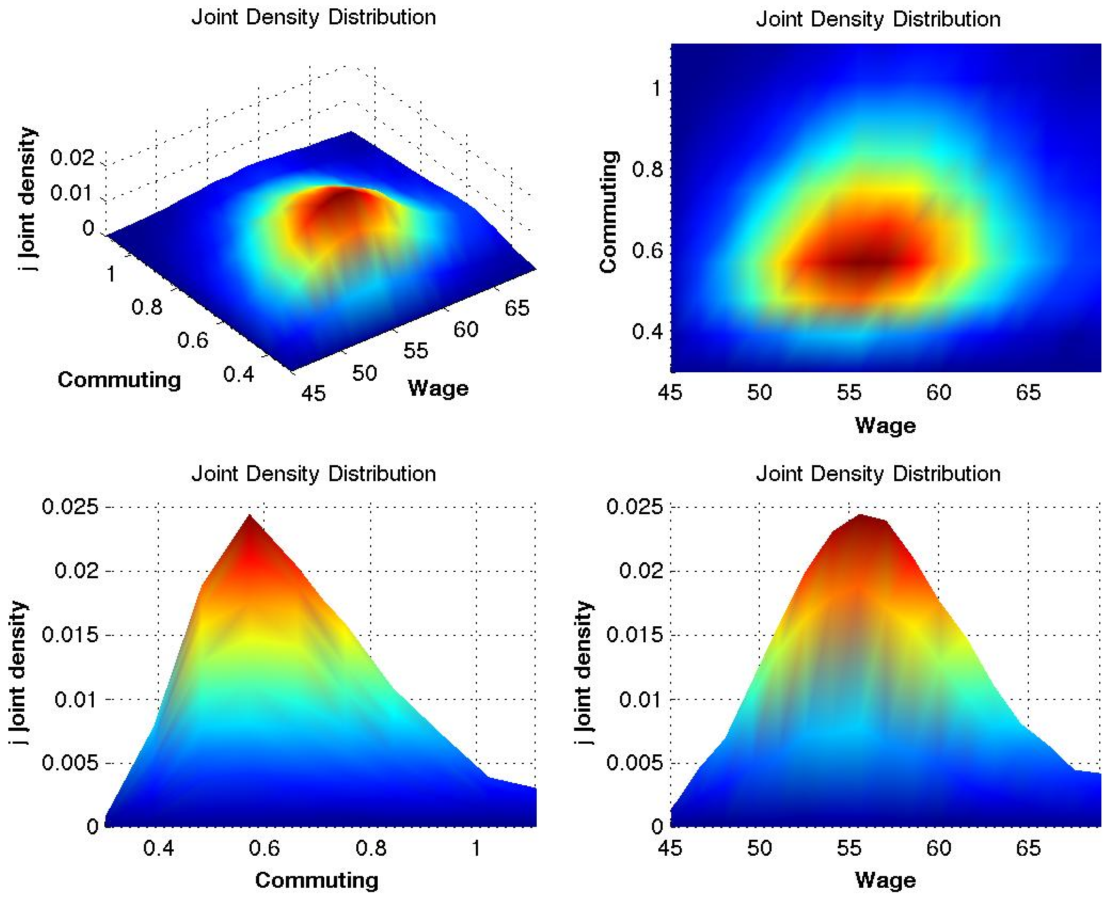
We finally report the conditional densities of the sample in the cross section of accepted jobs, in Figure 3.6. The joint density of accepted wages and commuting times shows a peak at 57 Euros wage per day, and about 32 minutes of commuting time (top left and right sub-graphs). Jobs that offer higher wages and longer commutes, or lower wages, and shorter commutes are also quite frequent. This is the pattern we saw in the previous figure.

Figure 3.5: Changes in Commuting Time and Wage



Notes: The graphs on top show the distributions of changes in commute distance in hours and wages in Euros omitting the spike at zero change in commute distance. The lower figure shows change in log wage against change in commute time. The sample is split whether individuals find a job before or after unemployment benefits have run out. Source: own calculations.

Figure 3.6: Joint Density Distributions for Commute Time and Wage



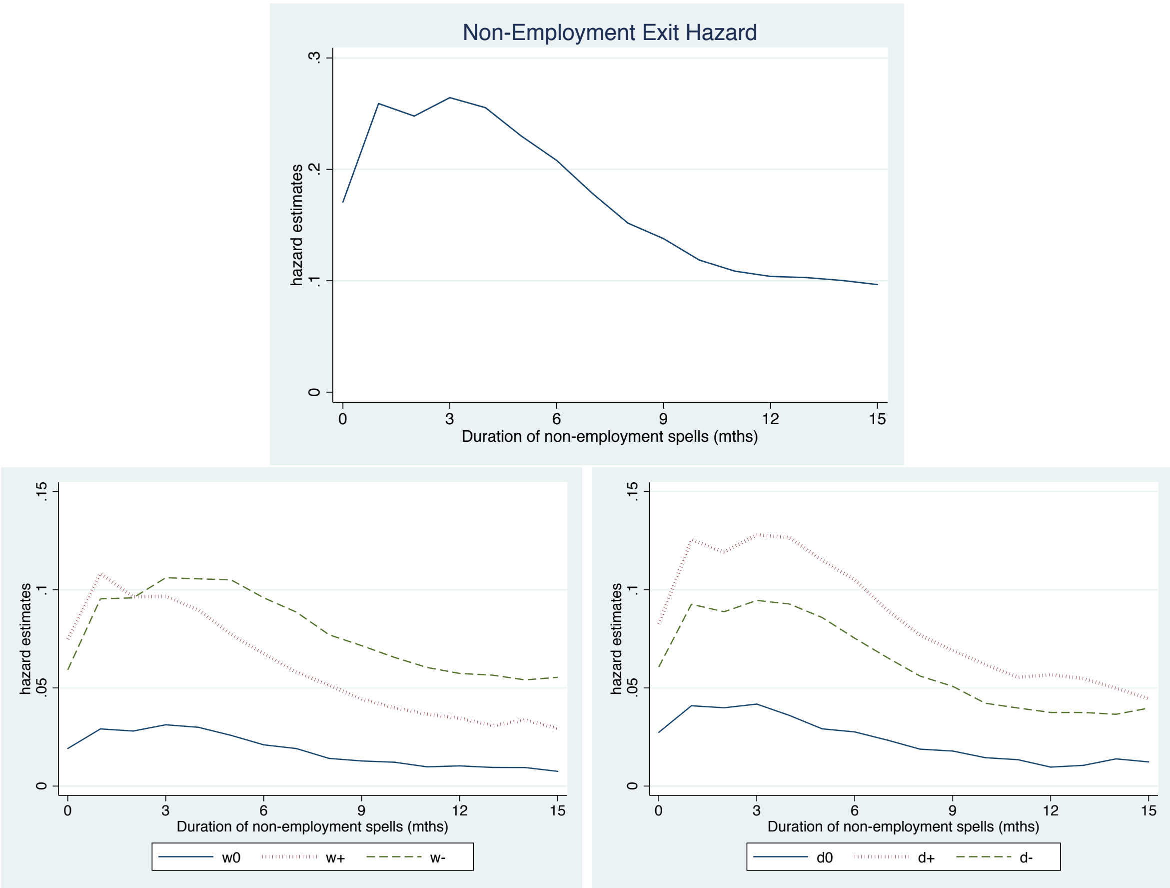
Notes: The figure shows the same data from Figure 3.5 but as joint distributions. Commuting is measured in hours, wages are measured in Euros. Source: own calculations.

3.3.4 Empirical Hazard Rates and Competing Risks Analysis: More Wage Cuts and Less "City Stayers" Over Time

With similar notations as in the theory part and in Table 3.2, we separate out transitions of workers towards a larger distance job ($d+$), those staying in the same city (d_0) and finally those facing a decline in commute distance ($d-$). Similarly for wages, we separate out workers facing transition to a higher wage ($w+$) and a lower wage ($w-$) and define transition to the same wage (w_0) if the new wage is within a range of 4% around the old wage.

The results are presented in Figure 3.7; it displays the profile of the hazard rate for the non-employment duration in the data. The unemployment exit hazard rate reaches a maximum between two to six months before it declines continuously. This could be because job seekers entering unemployment apply for jobs right away but need to wait until they receive a job offer. This is true for overall exits (top chart) and for each of the destinations (middle and lower chart).

Figure 3.7: Empirical Hazard Rates by Exit State



Notes: Figures report Kaplan-Meier estimates for the non-employment exit hazard. w stands for wage, d stands for commuting distance, + stands for increase compared to pre-unemployment, - stands for decrease to pre-unemployment. d_0 indicates that there is no change in distance. w_0 indicates changes in the wage of $\pm 4\%$. Source: ASSD, own calculations

Our theory for job search in space predicted that relative hazards inform on search strategies. We now establish a few stylized facts related to the "competing risks", to assess how the different sub-hazards relate to each other over time. We proceed as follows. We first estimate sub-hazards using Cox-Regression defined by the type of job an individual finds[11]. We distinguish better paying jobs, worse paying jobs, and about equal paying jobs. The about equal paying category means the new wage is up to 4% above or below the previous job. We introduce this category to deal with the issue that we do not know for sure whether job seekers would accept the previous job. In a second step, we build

[11]Note that doing so does not mean we split the sample by wage or distance, the type of job an individual finds merely defines which sub-hazard this individual contributes to estimating. We follow standard practice in competing risks estimation.

relative hazard rates. For instance, we calculate the relative hazard of wages by dividing the hazard estimate for $w-$ by the hazard estimate for $w+$ telling us how the relative probability to end up in relatively worse jobs behaves over time. The same can be done with distances.

The relative hazards are illustrated in Figure 3.8. Each plot includes the unconditional relative hazard ratios (black lines in the graphs), as well as the hazard ratio after controlling for some observable characteristics (red solid line). The latter is a prediction from a Cox-Estimation where we control for a variety of observed characteristics presented in Table 3.2. The black dashed lines are the corresponding 95% confidence interval.

The upper left panel relates exits in worse paid jobs to exits in better paid jobs. As expected, the relative likelihood that individuals leave into worse paid jobs increases with the duration of non-employment. This is evidence that reservation wages are declining over time, consistent with job search theory when workers loose eligibility. Degen (2014) finds a very similar pattern for accepted wages in Austria. This result, well known, is in line with a large body of evidence in other countries. Further, the left panel in the second row shows that this arises mostly from strong wage cuts: the relative hazard $w-/w_0$ goes up, while the left panel in the third row shows stability over time of $w_0/w+$.

The upper right graph relates exits into jobs farther away to jobs that are closer to home. Both the unconditional and the conditional relative hazards are almost flat. This implies that the succeeding job can be either closer or farther away from home. This ratio is 1.5 and stable over time, meaning that there is a larger fraction of distance losers ($d+$). This may be surprising since one would perhaps have expected, parallel to the decline in the reservation wage over time, that workers could face an increase in their reservation distance; this may suggest the absence of action along the distance margin. However, this interpretation is wrong, as indicated in the subsequent rows. The reason is not the insensitivity of the distance margin, but rather due to the fact that hazard rates away from the previous city actually evolve relative to the hazard of the "city stayers". This hazard rate account for the 16% of individuals in our sample who do not change the commuting distance in the new job as compared to the old job.

In fact, the unexpected result uncovered here is that the pattern of search *with respect to the previous city* varies quite a lot over time. Indeed, we obtain instead quite strong trends in relative hazard ratios where the denominator is the hazard rate of city stayers, as shown in the right panel in the second and third rows. The second row (right panel) relates exits into farther away jobs to exits into jobs at the same distance. Overall, there is a larger portion of unemployed individuals finding a new job farther away than staying in the same city. The proportion of "distance losers" ($d+$) relative to stayers (d_0) goes *up* over time. For workers experiencing such a move to a more distant city, this is indeed a change upward of the reservation distance strategy, that may be explained by a decline in the unemployment insurance. We also find a positive trend in time for the "distance winners" ($d-$) relative to stayers (d_0) (third row, right panel): individuals are indeed relatively more likely to find a job in the same place at the beginning of the non-employment duration than to move closer to home. This suggests that workers tend to search first for jobs in their previous workplace before searching jobs closer to home. As time goes however, some workers give in and get closer, possibly sacrificing on wages. Overall, it is relatively more likely to find a job in the same place at the beginning of the non-employment duration than towards the end of the non-employment duration.

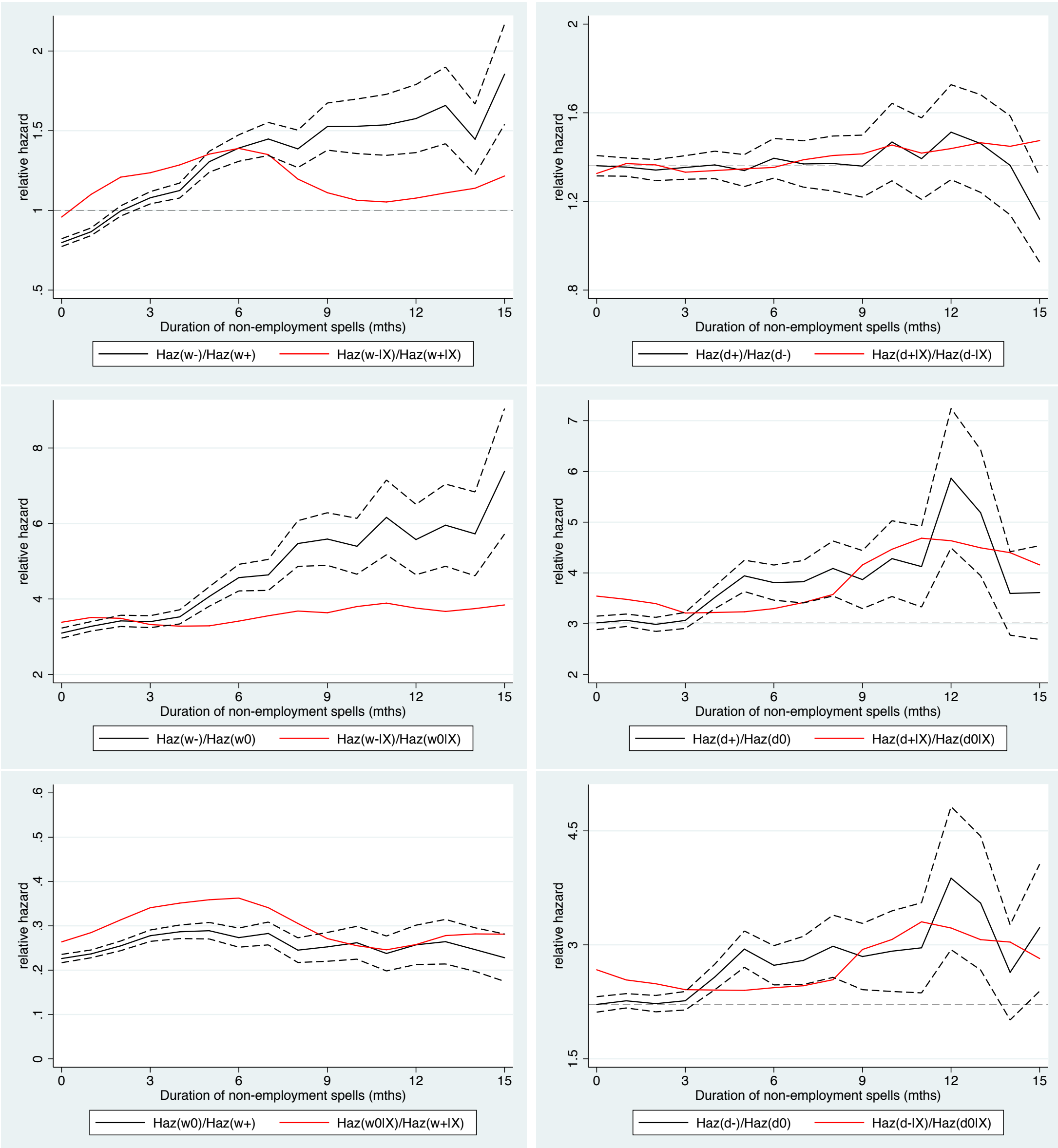
There are various possible interpretations of the above results, that the old workplace is a relevant margin for job search, especially at the beginning of the unemployment spell. Jobs are typically concentrated in space, e.g. finance jobs in the capital, and job seekers have work experience in only a few industries. Job seekers in spatially concentrated industries are more likely to find a job in the same city as before, until they change sector if unsuccessful. In that case, they also change their area of search and therefore move to another city. Another explanation would be that unemployed workers have more information about the old workplace e.g. through informal search channels. Both explanations can be true simultaneously and would produce the same observable consequences. We have explored these explanations by conducting the same analysis for workers who work in geographically clustered industries as opposed to workers who work in geographically uniformly distributed industries. We obtain similar results for both types of industries, suggesting that the information channel is important.

3.3.5 The Impact of Unemployment Benefits on Hazard Rates: Identification Strategy

We will estimate a basic Cox-model of the sub-hazard rates. Our particular focus here is on identifying the effects of three unemployment insurance parameters on the nature of jobs individuals accept. The identification of the effects of unemployment benefits, benefit duration, and unemployment assistance is obtained as follows.

First, unemployment benefits are determined by previous earnings. The benefit schedule exhibits two kinks as in Card et al. (2015), one at the bottom of insured earnings and one at the top of insured earnings. Conditional on previous earnings and other observables, the remaining variation in unemployment benefits mainly stems from the presence of the kinks. If individuals cannot manipulate previous earnings to shift themselves beyond one of the kinks, the variation in unemployment benefits generated by the kink can be assumed to be exogenous. Importantly, the earnings that constitute the benefit base are not necessarily the ones where the job was lost. The relevant earnings to determine

Figure 3.8: Relative Conditional Hazard Rates from Empirical Data Analysis



Notes: Figures report relative Kaplan-Meier estimates for the non-employment exit hazard. w stands for wage, d stands for commuting distance, + stands for increase compared to pre-unemployment, - stands for decrease to pre-unemployment. d_0 indicates there there is no change in distance. w_0 indicates changes in the wage of $\pm 4\%$. The black solid lines with the 95% confidence interval are unconditional relative hazard rates. The red solid lines are relative hazard rates from Cox estimations including the full set of controls listed in Table 3.2. Source: ASSD, own calculations.

unemployment benefits are either from the previous year or two years before, depending on when the individual starts claiming unemployment benefits. It is hardly possible for job seekers to manipulate the relevant previous earnings that ultimately determine the level of unemployment benefits.

Second, similar reasoning holds for the potential duration of unemployment benefits (PBD). PBD depends on previous work experience and age with discontinuous changes after several work experience thresholds, and two age thresholds (40 years and 50 years). Our strategy to exploit those changes is to add flexible functions of previous work experience and age into the Cox-regressions. Appendix Figure 3.23 documents the non-linearities used in the strategy. Recall that the coefficient of a regressor in the multiple regression model is the partial correlation of that regressor with the dependent variable. The bottom graph of Figure 3.23 shows PBD after all regressors in the model, including work experience and age, have been partialled out. The residual is close to zero almost everywhere, except at age 40 and age 50. PBD exhibits discrete jumps at these ages, thus identifying the coefficient on PBD. PBD effects are identified from the age and previous work experience discontinuities in PBD.

We are not aware of a quasi-experimental design for unemployment assistance. We use the observed level of unemployment assistance conditioning on some potential determinants of unemployment assistance receipt (marital status, previous wage).

3.3.6 Evidence of Disincentive Effects

Table 3.3 displays the effects of the level of benefits from unemployment insurance B and from assistance b on hazard rates. Column 1 displays the results while controlling for the effect of benefits under the UI regime (B) and potential benefit duration (PBD). The sign on the hazard rate is strongly negative. The effect of potential benefit duration is also negative and significant. The regressions include a number of other factors, including tenure profiles, marital status and family composition, as well as provincial dummies (NUTS3), industry dummies, altitude, and year effects[12].

The second column introduces further the value of unemployment assistance (b) for those having exhausted their UI rights. So B measures the replacement rate for job seekers on UI, and b is the replacement on unemployment assistance for job seekers on assistance. In this specification, potential benefit duration captures the number of weeks remaining before exhausting benefits. Both levels of UI (B) and UA (b) reduce the hazard rate, although the effect of b is smaller than B. The effect of PBD is still negative but less so.

[12]Table 3.15 in the appendix shows the full set of covariates.

The next columns investigate which sub-hazards are more strongly affected by changes in the unemployment insurance parameters. Making UI more generous should not affect exits to good jobs (paying a higher wage), except via reduced search intensity. Indeed, point estimates for UI benefits and assistance are small in column ($w+$). More generous unemployment insurance makes exits to jobs that pay the same or worse much less likely (columns $w-$ or w_0). So, making UI more generous improves chances that job seekers find a better paid job, *relative* to finding a worse paid job.

Regarding the links between UI and distance, one would expect more generous UI to reduce the rate of leaving to jobs further away from home (column $d+$). This is true for potential benefit duration which reduces the rate of accepting jobs far away from home. However, for benefits, we do not see that increasing UI reduces exists to jobs further away from home. Instead, increased UI reduces the rate of leaving for a job in the same city (column d_0), relative to jobs closer or farther away from home. This might be because increased UI facilitates job search in new areas. UI benefits enlarge the search radius around the previous city. All estimates attached to UI generosity display negative signs, this reflecting the effect of UI on search intensity. The impact of UI and UA on joint wage and distance changes is displayed for completeness in Appendix Table 3.16.

Table 3.4 offers a summary of the differential effects of benefits and assistance on changes in distance and wages, where the reference is staying in the same city and at a wage within the -4%/+4% range. Interestingly, benefits and assistance raise significantly the occurrence of the outcome "higher wage"; benefits reduce the occurrence of the outcome "lower wage", assistance being insignificant here. Further, netting out the $d-$ coefficients to the d_0 coefficients, it appears that benefits increase the likelihood to get closer to home than staying in the same city; and, for wage increases and wage stability (first two rows), netting out the $d+$ coefficients to the d_0 coefficients implies that benefits increase the likelihood to get further away to home than staying in the same city.

In summary, in a majority of cases, unemployment insurance reduces reservation wages at a given distance, and promotes search outside the same city, especially closer to home, as expected, but also further away, which is per se a less expected result. Appendix D explores whether this may be due to credit constraints. Evidence lightly points out in this direction (see Appendix Table 3.19).

Table 3.3: Cox-Model Estimates Hazard Rates: All Destinations, by Wage Change and by Commute Distance Change

	(all)	(all)	($w+$)	($w-$)	(w_0)	($d+$)	($d-$)	(d_0)
B	-0.542***	-0.640***	-0.350***	-2.494***	-1.475***	-0.537***	-0.652***	-0.910***
	(0.031)	(0.026)	(0.036)	(0.054)	(0.063)	(0.037)	(0.040)	(0.060)
b		-0.271***	-0.314***	-0.589***	-0.522***	-0.261***	-0.289***	-0.258***
		(0.033)	(0.052)	(0.044)	(0.100)	(0.047)	(0.052)	(0.082)
PBD [weeks]	-0.003***	-0.001	-0.004**	0.006***	-0.000	-0.004***	-0.000	0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.001)	(0.002)	(0.002)
Nuts3 FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spells	154,677	154,677	154,677	154,677	154,677	154,677	154,677	154,677
Individuals	118,343	118,343	118,343	118,343	118,343	118,343	118,343	118,343
Log L	-1613517	-1613357	-666060	-737931	-196332	-748354	-591330	-266454
Share Exits	0.96	0.96	0.40	0.44	0.12	0.44	0.35	0.16

Notes: Duration variable is nonemployment in months. Estimates refer to coefficients. w_0 contains changes in wage of $\pm$ 4%. Control Variables: Potential Benefit Duration, net wage used for calculation of replacement rate. Experience in the last two, five and ten years (5 is net of 2 and 10 net of 5 years), altitude of the municipality of residence, time to the next large city, age in years, real wage and occupation of the last job before unemployment, marital status and number of children. Voluntary quits and recalls are excluded, only Replacement Rates weakly below 1 and potential benefit durations above 0 are considered. Standard errors are clustered on individual level. Significance is indicated as follows: * (p<0.01), ** (p<0.05), *** (p<0.01).

Table 3.4: Coefficients from Table 3.16, Relative to Exit (w_0, d_0)

	Benefits			Assistance		
	$d+$	d_0	$d-$	$d+$	d_0	$d-$
$w+$	1.49***	1.177***	1.508***	0.529**	0.533*	0.539**
w_0	0.454***	0	0.348**	0.491*	0	0.226
$w-$	-0.694***	-0.585***	-0.758***	0.22	0.412	0.241

Notes: The table summarizes estimates from a competing risk Cox regression to each combination of wage and distance destination. The table reports coefficients on unemployment benefits (B) and unemployment assistance (b) relative to the coefficient estimated for the constant wage and same municipality of residence destination (w_0,d_0). Significance is indicated as follows: *(p<0.1), **(p<0.05), *** (p<0.01).

3.4 Extending the Model to Account for the Empirical Facts

This Sections enriches the model to take stock of these finding. It adds several dimensions to the previous analysis in Section 3.2. In particular, it shows how to account in a simple way:

1. for the existence of potential credit market imperfections;
2. for the local dimension of job search, and the particular role of the previous workplace that seems to be central in the Austrian case;
3. it also extends the model to the existence of two unemployment compensation profiles, insurance and assistance.

3.4.1 Mild Liquidity Constraints, Unemployment and the Role of Benefits

The previous results were derived under the assumption that agents face no liquidity constraint. Under the assumption of a search cost taking the form $C(D, \lambda) = M(D) + e(\lambda, D)$, where the first part may be thought as a monetary component and the second part as disutility of effort and distance, this requires that the income from benefits and other assets is larger than the financial cost, or that the unemployed workers may borrow at the same rate as the employed workers save. Indeed, this assumes that the rate of interest r is the same for borrowers (the unemployed) and savers (some of the employed). Another "almost equivalent" assumption is that the unemployed workers who have just been laid-off either still have financial assets or full access to financial liquidity. In that case, the situation of the newly unemployed workers is similar to that of the employed workers, which was our working hypothesis so far.

We represent this alteration with the assumption that the newly unemployed workers have access to the same rate of interest for a random time, and under some Poisson intensity process, undergo a drop in their financing capacity.

In that case, a *mild liquidity constraint* is that they face a higher interest rate r^+ but may still borrow at this rate and therefore, choose the optimal range of search. Another extreme assumption is that these unemployed workers, after being hit by a financial constraint, cannot even borrow and face a *strict liquidity constraint*, under which their current income must equal their spendings: consumption and monetary search costs. We do not detail the model solutions in this case since we do not find strong evidence in favour of such strict constraints in the data and only leave this for the Appendix (sub-section B.2). These unemployed workers must now discount the future at their rate of pure time preference, and r^+ must now be interpreted as such a rate, going say from 4% a year to 20% a year.

In other words, the newly unemployed workers are decumulating assets and make optimal search decisions; following a financial shock unemployed workers have no longer any asset and must either borrow at a higher rate or face cash-constraints and discount the future at their rate of time preference.

Lemma 3 (**unemployment benefits impact**). *i) In the absence of liquidity constraints, an increase in unemployment benefits increases the value of unemployment by a factor* $1/r$*. ii) Under mild liquidity constraints, the impact is* $1/r^+$ *and thus smaller.*

The proof of the impact of unemployment benefits on the value of unemployment is also in Appendix B.2 in all possible cases.

3.4.2 Introducing Two Levels of Unemployment Compensation

Now, we assume that there are two levels of benefits: B (insurance) and b (assistance). Workers switch randomly from B to b at Poisson rate α[13]. The value of unemployment depends on the eligibility status; let U_c and U be these values for workers covered by UI and by UA, respectively, and ρ the commute distance. Let λ and λ_c be the arrival rates of job offers per unit of superficy, and first simplify the exposition in treating λ and λ_c as simple parameters. As already shown in Section 3.2, the optimal values of λ can be easily calculated once the optimal search radius D^* has been chosen.

We also assume that the financial constraint of the unemployed gets more severe as time goes. However, instead of assuming that agents can accumulate and decumulate

[13]Many real world UI systems are not stationary, e.g. unemployment benefits run out after a fixed number of months. Non-stationarity can matter for job search behavior, as studies on benefit exhaustion show (Meyer (1990)). Card et al. (2007b) discuss end of benefit behavior and find it matters much less than earlier studies would suggest. Our specification buys us simplicity at a reasonable cost.

wealth, we make the simplifying assumption, already discussed in Section 3.4.1, that individuals face a higher rate of discount after a Poisson shock; although in principle the loss of eligibility to unemployment insurance and the more difficult access to liquidity are distinct stochastic processes, we assume that they occur simultaneously, which simplifies the derivation of the model. Then, we simply assume that the covered unemployed workers access to credit at rate r_c, which is lower than that the rate r faced by the uncovered workers. We also assume that search effort dimensions (here D only) and consumption are non-separable, with an interaction term proportional to parameters δ and δ_c; δ (δ_c) positive (negative) means that the disutility of distance is lower (higher) for higher income recipients. The full derivation of the extended model can be found in Appendix A.2.

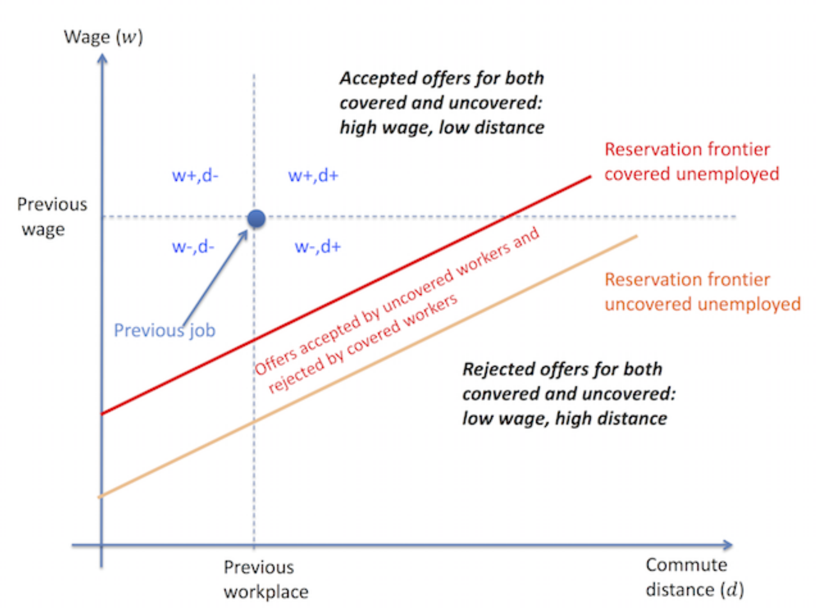
The following Lemma highlights how job seekers change their reservation wage when switching from the UI to the UA regime.

Lemma 4. *Assuming $\delta = \delta_c$ and $r = r_c$, the reservation wage for a given distance is higher for eligible unemployed workers than for ineligible workers. The difference is in(de)creasing in commute distance if $\delta < (>) 0$.*

$$R_c(\rho) - R(\rho) = \frac{r_c + s}{1 + \delta\tau\rho}(U_c - U) > 0. \tag{3.7}$$

We can grasp the main intuition by focusing on the simple case with separability between monetary income and distance and linear commute distance cost function. In this case, we already proved that reservation wages are linear in the commute distance and the marginal rate of substitution is constant, denoted by τ. In this case, the linearity comes from the fact that wages enter linearly in the utility function and that commute costs are linear in distance. It follows that the reservation frontier in wage and distance is linear, and can be represented as such in Figure 3.9.

Figure 3.9: Theoretical Reservation Frontiers and Acceptance-Rejection Areas



3.4.3 Directing Search Towards the Previous City

The main insight of the previous empirical part is that workers seem to search first in the previous city, and then extend their range of search. We want to give a theoretical counterpart to this complex job strategy. Assume now that workers can target the effort strategy λ differentially in space, contrary to what was assumed before. To keep things relatively simple, we assume that workers can distribute their search effort either in the previous city (with intensity of arrival of offers λ^0) or in any other city within the range D (with intensity of arrival of offers λ). Because space is continuous in our setting, we define the previous workplace as a range of values centered on the mean of the distance distribution (d_0): the lower and the upper bounds of the range are denoted as $d_{0^-}$and d_{0^+}, respectively.

The optimal search strategy is therefore six-tuple (D, D_c, λ^0, λ^0_c, λ, λ_c). The first order conditions for the optimal search radius stay as in the benchmark model (see equation 3.18 and 3.19). The new first order conditions on optimal search intensity are reported in Appendix A.3.

The specification we adopt for the cost functions is the following:

$$C(D, \lambda, \lambda^0) = \tau D + c^0 D^{\eta_c} + c^\lambda \left[\gamma^{\lambda^0} (\lambda^0)^{\eta_\lambda} + (\lambda)^{\eta_\lambda}\right]$$

$$C'_{\lambda}(D, \lambda, \lambda^0) = c^{\lambda}\eta_{\lambda}(\lambda)^{\eta_{\lambda}-1}; \quad C'_{\lambda^0}(D, \lambda, \lambda^0) = c^{\lambda}\gamma^{\lambda^0}\eta_{\lambda}(\lambda^0)^{\eta_{\lambda}-1};$$

As regards the part of the search cost which depends on distance (D), we assume that it is made by two components: the first one is a monetary component, and the second one is a convex function which represents agent's disutility from searching farther away from residence. The cost of search effort only presents a convex disutility component. As discussed in Section 3.4.1, the monetary component of the search cost (summarized by τ) enters the agent's budget constraint. In this way we can study the case of binding liquidity constraints, which leads to sub-optimal choices of the radius of search. Regarding the disutility component, c^0 and c^{λ} are the weights of the distance and the effort dimensions, respectively. η^c and η^{λ} are the elasticities of the subjective part of the cost function to these two search margins. Furthermore,γ^{λ^0} captures how costly is the search effort in the previous workplace relatively to search outside. We assume $\gamma^{\lambda^0} < 1$ to indicate that the search efficiency is likely to be larger in the previous workplace, either for industry concentration or for existing social networks.

Furthermore, covered workers are assumed to be relatively more efficient in searching in the previous workplace ($\gamma_c^{\lambda^0} < \gamma^{\lambda^0}$): in absence of other dimensions of heterogeneity, the asymmetry in the search cost is needed to rationalize the empirical observations that covered workers exit unemployment more quickly and they are relatively more "city stayers". Moreover, there are several empirical reasons that may justify this choice: shorter non-employment spells are often associated with a richer human and social capital and are considered as a positive signal by potential employers.

3.5 Calibration of the Richer Model and the Role of Policy Parameters

3.5.1 Calibration Parameters and Summary of the Main Variables

As Figure 3.7 showed, the hazard rates decrease over time. This may arise due to: i) discouragement from job seekers as time goes - e.g time varying search costs; ii) lower quality of job offers due to the exhaustion of offers in the initial pool of search (e.g. the same city); iii) a stigma effect from being long-term unemployed and thus less efficient search as time goes; iv) more impatient workers over time, hence reducing their search effort; v) illiquid workers who cannot afford paying for the optimal search effort and who restrict their range of search; vi) finally, heterogeneity of workers and a composition effect in the pool, so that those less efficient dominate over time. Mechanisms ii) and iii) are for instance assessed in Kroft et al. (2013), who find a negative association between the length of elapsed unemployment spells and the likelihood to obtain a job interview.

We therefore enrich the model with a set of assumptions encompassing these various mechanisms and consistent with these interpretations. Assume the existence of two types of unemployed workers: covered workers are entitled to benefits B, and uncovered workers are assistance recipients $b < B$. Covered workers are assumed to face a relative higher efficiency of search in the same city, while uncovered workers face instead a less efficient search effort. This hypothesis captures the first three explanations of the declining hazard rate listed above. Additionally, covered unemployed workers face a lower rate of interest and are thus more patient and search *ceteris paribus* more; the uncovered, under assistance, face a higher rate of interest and search less. This assumption is consistent with previous point iv). Appendix B.2 extends the model in the direction indicated by point v). We do not explicitly address point vi), instead. Hence, as time goes, we observe both a decline in the absolute hazard rate and, under adequate choice of the relative efficiency of search in the same city, a decrease over time of the hazard rate in the same city relative to the hazard rate outside the city.

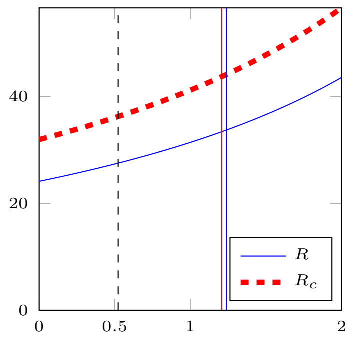
We then choose the various parameters so as to replicate the qualitative results on hazards, relative hazards and sub-hazards as in Section 2. The full calibration is reported in Table 3.5. The rate of interest is set to 4% annually for the employed workers and for the covered unemployed workers (under UI), and at 12% for the uncovered workers (under UA). The discount in the search cost of prospecting in the same city is $\gamma_c^{\lambda^0}$=0.07 for covered workers, but that comparative advantage of the previous city decreases for the uncovered workers and that discount parameter goes to γ^{λ^0}=0.14 instead. Further details on the calibration strategy are relegated to Appendix A.5.

Table 3.6 reports the main equilibrium variables of the model. The simulated reservation frontier, the counterpart of the theoretical Figure 3.1, is instead represented in Figure 3.10: since we assume a negative $\delta = \delta_c$ and a linear cost function, the reservation frontier turns out to be convex. The blue and the red vertical lines represent the radius of search for uncovered and covered workers, respectively.

An outcome of the model is that covered workers ask for higher wages ($R_c > R$)[14], search closer ($D_c < D$) and search more intensely ($\lambda_c > \lambda$; $\lambda_c^0 > \lambda^0$). The higher search intensity of covered workers is due to their comparatively higher efficiency, as stressed in the previous section. This allows them to exit unemployment more quickly ($haz_c > haz$). Moreover, job seekers under the UI regime are more likely to find a job in the previous

[14]More exactly, covered workers have a higher reservation frontier: their reservation wage is higher for any given commute distance. The figures reported in Table 3.6 are the reservation wages calculated at D.

Figure 3.10: Simulated Reservation Frontiers



Notes: The vertical dashed line is the previous city (d_0); the vertical red (resp. blue) solid line is the optimal range of search of covered unemployed workers D_c^* (resp. of uncovered workers D^*). Distance is measured in hours.

workplace, as evident from the higher fraction of city stayers among covered workers.

3.5.2 Search Strategies, Hazard and Relative Hazards as a Function of Non-employment Spells

Figures 3.11 and 3.12 plot the results of the simulations. The model performs relatively well under different dimensions. First, we are able to replicate the decrease in the absolute hazard and in the sub-hazard rates (Figure 3.11). Second, we match the empirical result that the share of workers exiting unemployment as wage losers is increasing over time (column one in row two of Figure 3.11). Third, the model can account for the fact that agents are more likely to expand the radius of search the more time they spend into unemployment (column 2 in row two of Figure 3.11). Fourth, agents exhaust job offers inside the previous workplace as time goes. In summary, the lower part of Figure 3.11 shows that the extended model qualitatively accounts well for the empirical dynamics of sub-hazards that were depicted in Figure 3.8.

The underlying mechanisms of the model are represented in Figure 3.12: as time goes, the reservation wage R goes down and the search radius D on average increases. This happens because covered workers search closer and are more picky regarding the wage. The right panel of Figure 3.12, however, shows a new finding: a large part of the action here also comes from the changes over time of the hazard rate for the category of "city stayers" d_0, that is people getting job offers in the same city where they used to work. This is an interesting finding, because it suggests that two spatial margins matter: a) the commute distance, based on search strategy D centered around the city of residence; b) the targeted search strategy λ but especially λ_0, that may temporarily be centered around the previous city of work.

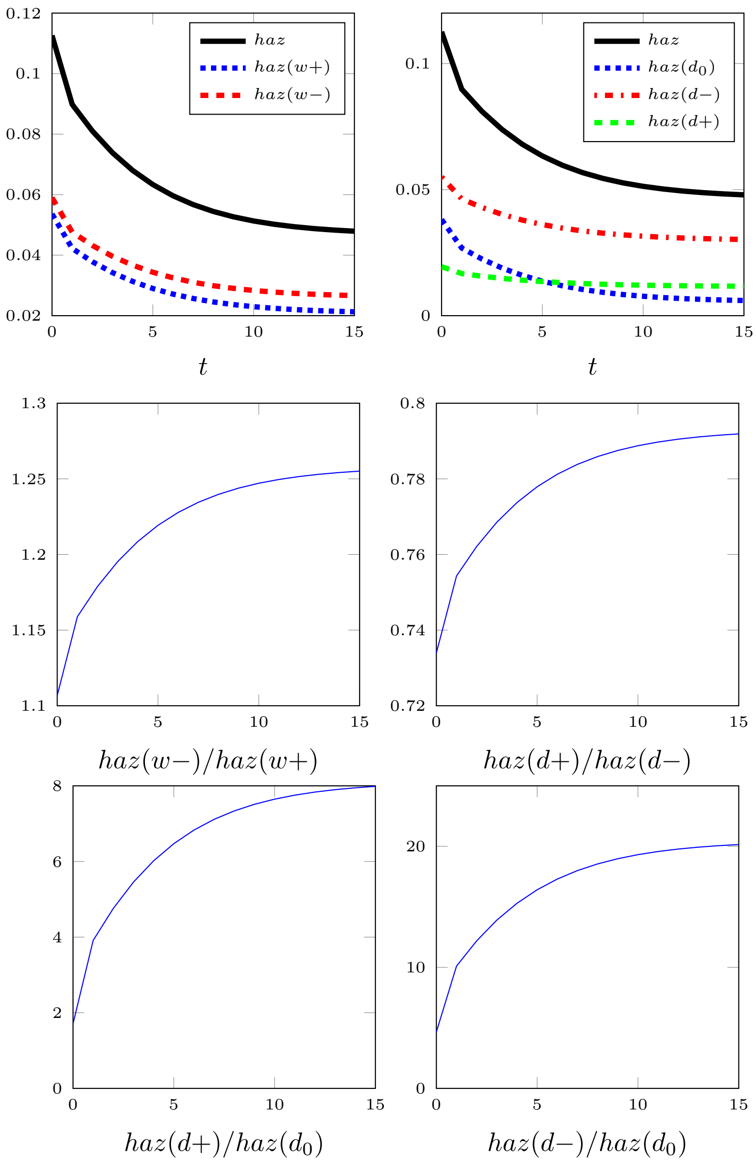
Table 3.5: Calibration

Parameter	Description	Value
r	discount rate	0.01
s	separation rate	0.004
c^0	cost of search (distance)	5.00
$c^\lambda = c_c^\lambda$	cost of search effort	80000
η_c, η_λ	elasticity of the search effort cost	1.50
γ_{λ^0}	cost of search in the same city	0.07
$\delta = \delta_c$	complementarity between income and distance	-0.20
Policy parameters		
B	Unemployment Insurance (UI)	20.59
b	Unemployment Assistance (UA)	1.76
$1/\alpha$	Potential Benefit Duration	5.00
τ	unit commuting cost	1.00
Wage and distance distributions		
$\mu^F = \mu_c^F$	mean wage	58.84
$\sigma^F = \sigma_c^F$	sd wage	18.90
$\mu^G = \mu_c^G$	mean distance	0.47
$\sigma^G = \sigma_c^G$	sd distance	0.47

Table 3.6: Main Endogenous Variables of the Calibrated Model

	Covered	**Not covered**
Observed outcomes		
Average wage (euros per day)	64.75	60.25
Average distance (min)	25.51	25.84
Hazard rate	0.1126	0.0467
Rejection rate	0.012	0.003
Share city stayers	18.88	5.12
Unemployment	0.012	0.051
Decisions		
Reservation wage	43.63	33.03
Search radius (min)	71.08	73.84
Effort outside d_0	0.0030	0.0020
Effort inside d_0	0.0200	0.0100
Sub-hazard rates		
$sub - haz(w+, d+)$	0.0098	0.0052
$sub - haz(w-, d+)$	0.0096	0.0064
$sub - haz(w+, d-)$	0.0256	0.0131
$sub - haz(w-, d-)$	0.0295	0.0166
$sub - haz(w+, d_0)$	0.0182	0.0024
$sub - haz(w-, d_0)$	0.0199	0.0030

Figure 3.11: Simulated Hazard Rates from the Extended Theory.

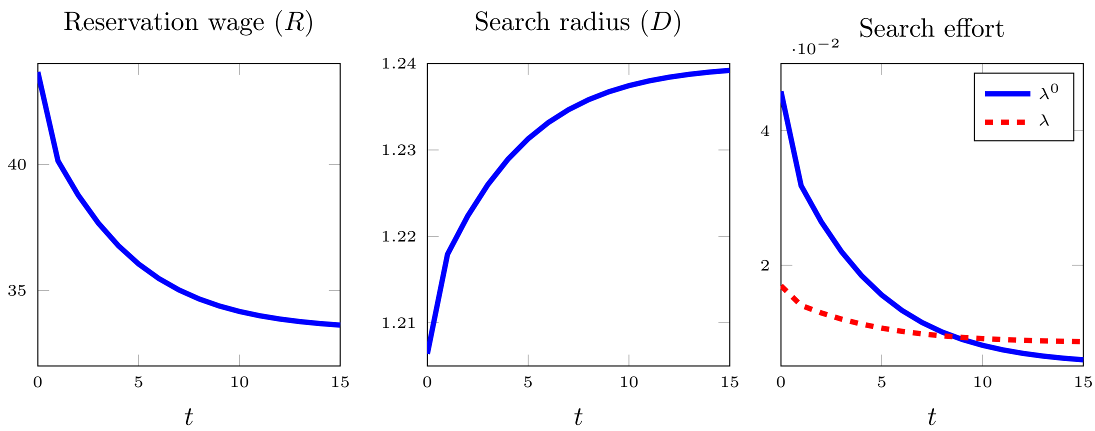


Notes: Black solid line: total hazard. Dashed colored line: sub-hazards, summing up to total hazard.

3.5.3 Comparison of Calibration Moments with the Data

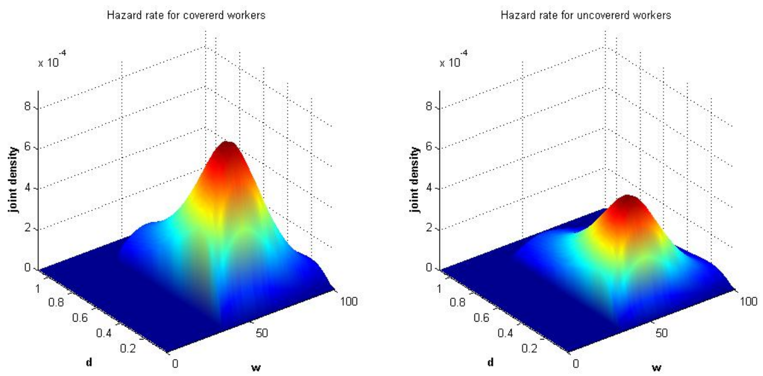
The map of densities of hazard rates in the cross-section of unemployed workers represented in Figure 3.13 in the distance-wage space, for both covered and uncovered workers, are quite similar to the equivalent empirical densities in Figure 3.6. One can also represent the "predicted" accepted wages in the model for both covered and uncovered workers. This corresponds to the solid and dashed lines in Figure 3.14. The solid line for covered workers is close to the empirical observations. The dashed line for uncovered workers is higher compared to the empirical observations. This suggests that our model may cap-

Figure 3.12: Search Strategies from the Extended Theory: Reservation Wage, Search Radius and Relative Intensity of Effort in the Same City (λ^0) relative to other cities (λ)



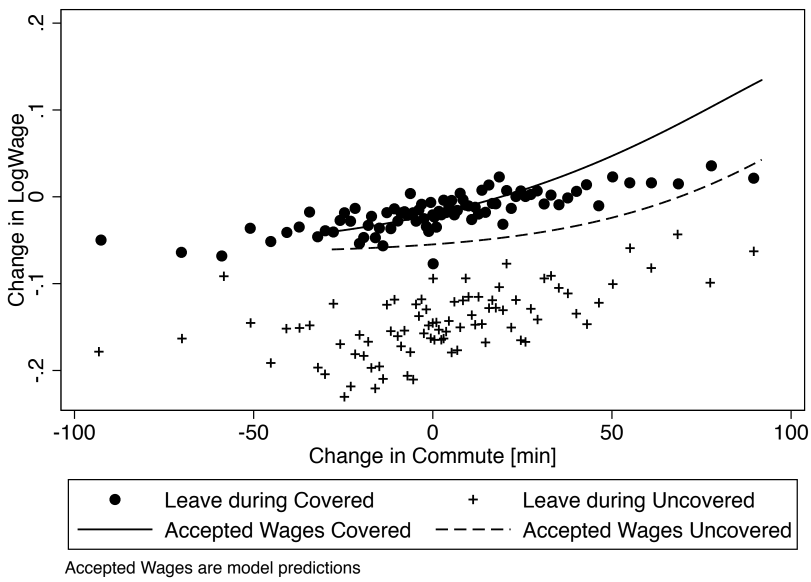
ture well the effects of unemployment insurance but less so the effects of unemployment assistance. This gives room for improvement of the calibration exercise, introducing more ex ante heterogeneity in the pool of uncovered workers (those under UA).

Figure 3.13: Joint Density Distributions from Simulations of the Extended Theory



Notes: Commute Distances are measured in hours. Daily wages are measured in euros.

Figure 3.14: Changes in Commuting Time and Wage



Notes: Empirical data is shown as dots (covered) and stars (uncovered). Simulations results from extended theory are shown as solid (covered) and dash (uncovered) lines.

3.5.4 Policy Implications from the Calibration

The next stage is to describe the comparative statics of unemployment insurance for agents subject to mild liquidity constraints. In what follows, we will explore systematically the comparative statics of B, b and Potential Benefit Duration $(1/\alpha)$ on the main endogenous variables of the model, the reservation strategies and distance search as well as employment and unemployment. In Appendix B.2 we further consider the case of agents who are strictly liquidity constrained and compare the difference in the search behaviour implied by the calibration exercise.

Figure 3.15 shows the response of the main variables of the model to a variation of the policy parameters, namely the unemployment insurance enjoyed by covered workers (B), the unemployment benefit received by workers who lost the insurance (b) and Potential Benefit Duration (PBD).

Variation of B and PBD often have opposite effects on covered (on UI) and uncovered (on UA) workers. Increases in B and PBD make the covered workers choosier: they decrease their radius of search and their reservation wage increases. Furthermore, they reduce the search intensity both inside and outside the previous workplace. The joint effect is a reduction in the hazard rate for covered workers. On the contrary, B has no disincentive effect on uncovered workers, since they do not actually receive it. We can observe a mild entitlement effect instead: B raises the value of re-employment and therefore the effort made by uncovered workers to find a job. The result of a larger B on uncovered workers is therefore that D increases as long as the search effort increases, while R decreases. As a result, uncovered workers are more likely to exit unemployment. Changes in b makes both types of workers choosier, leading to a reduction of the hazard rate for both types of searchers.

Table 3.7 presents the elasticities of outcomes and decisions with respect to the parameters of the unemployment insurance system. Consider, first, the effects of increasing the unemployment benefit level for covered job seekers. Accepted wages and commuting distance display only a small reaction, but the unemployment exit hazard decreases, so unemployment duration increases, and job seekers reject more wage offers. Why does unemployment duration increase? The reservation wage barely increases, explaining the small increase in rejections, but the search radius decreases substantially. Moreover, job seekers search less, both inside the previous workplace, and outside it.

Changes to the duration of unemployment benefits also affect covered job seekers directly. Increasing the duration of benefits increases wages somewhat, and reduces commuting distance. The unemployment exit hazard decreases substantially, rejections increase, more people work in the previous workplace, and more people are unemployed. Job seekers increase their reservation wage strongly, and decreases their search radius. Search intensity also plummets, both inside and outside the previous workplace.

Changes in unemployment assistance, b, affect covered job seekers only once their benefits have run out. Covered job seekers react to unemployment assistance changes in a way that mimics unemployment insurance, B, but elasticities are smaller because job seekers discount the future changes in unemployment assistance. Forward-looking job seekers do take changes to the social assistance level into account.

Table 3.7 also shows results for uncovered job seekers. Changes in unemployment assistance affect uncovered job seekers directly. Assistance levels have small effects on accepted wages, and distances, but lower the unemployment exit hazard considerably. Uncovered job seekers reject more wage offers, and unemployment increases, once the unemployment assistance level is increased. Uncovered job seekers leave unemployment less quickly because they search less, both inside and outside the previous workplace. The reservation wage increases somewhat, and the search radius decreases, but the elasticities are a bit smaller than the effort elasticities.

Uncovered job seekers could also be affected by changes in the benefits levels and durations of covered job seekers because, by leaving unemployment, uncovered job seekers gain entitlement to regular unemployment benefits. Yet, the entitlement effect of raising the benefit level is small for uncovered job seekers. Elasticities are essentially zero.

Table 3.7: Elasticities from Simulations of the Extended Theory

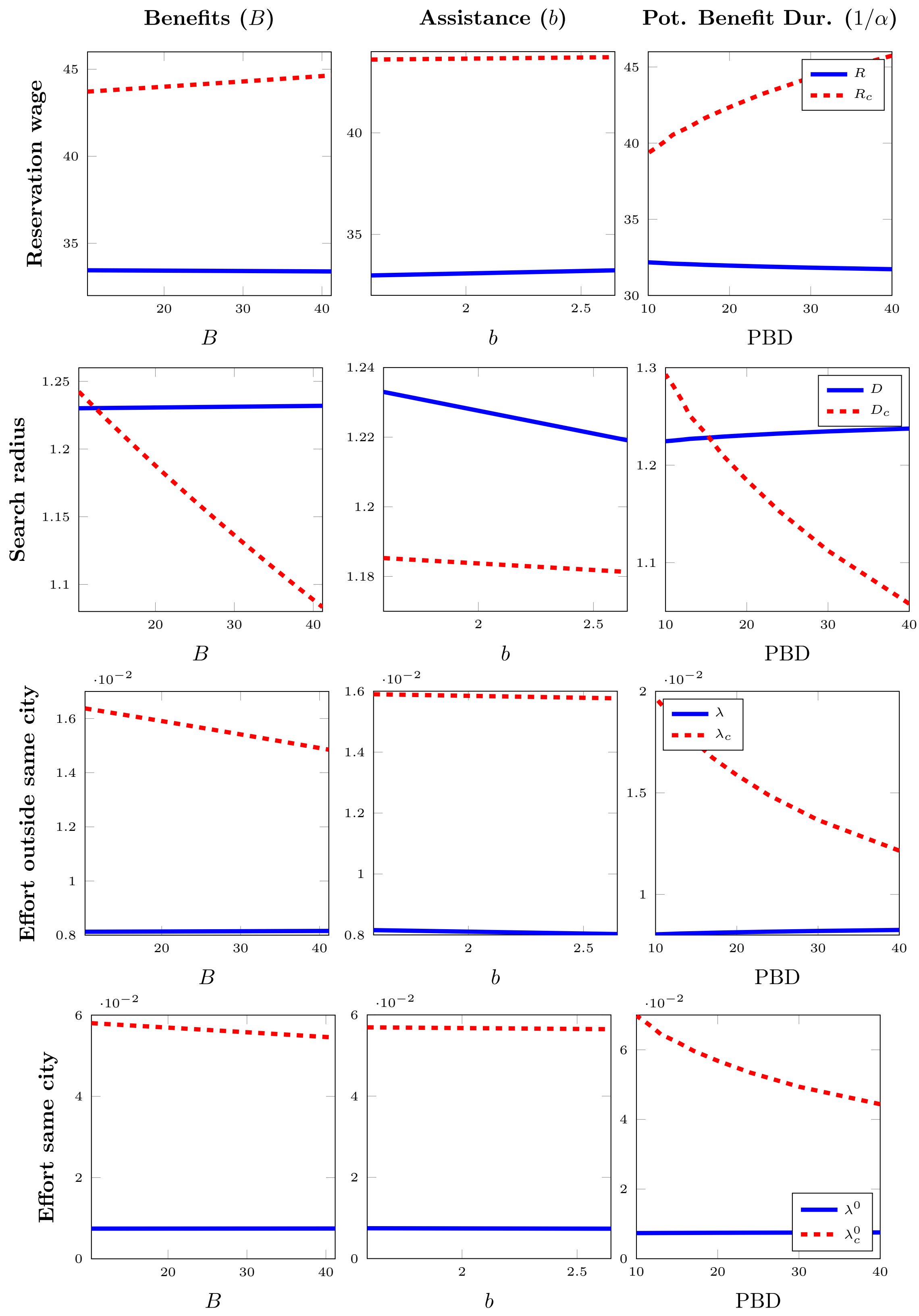
	Covered job seekers			**Uncovered job seekers**		
	B	b	PBD	B	b	PBD
Observed outcomes						
Average wage	-0.00707	0.00091	0.02100	-0.00017	0.00081	-0.00105
Average distance	-0.03718	-0.00182	-0.05775	0.00038	-0.00591	0.00238
Hazard rate	-0.08693	-0.01569	-0.40646	0.00314	-0.02832	0.01993
Rejection rate	0.02072	0.01303	0.24616	-0.00884	0.06527	-0.05663
Share city stayers	-0.00718	0.00349	0.24564	0.04675	0.00327	-0.34502
Unemployment	0.02950	0.00384	0.75683	0.02634	0.03240	-0.20575
Decisions						
Reservation wage	0.01606	0.00418	0.09902	-0.00150	0.01181	-0.00957
Search radius	-0.10222	-0.00519	-0.14854	0.00112	-0.01752	0.00710
Effort outside d_0	-0.07389	-0.01310	-0.33641	0.00283	-0.02460	0.01796
Effort inside d_0	-0.04779	-0.01243	-0.31006	0.00264	-0.02078	0.01678

Notes: The elasticity of y with respect to x is defined as $\left(\frac{\Delta y}{y}\right) / \left(\frac{\Delta x}{x}\right)$. Elasticities are computed from simulations, considering the following policy changes: ΔB from 40 to 50 % of the previous wage; Δb from 8% to 10% of the benchmark B; Δ PBD 30 to 39 weeks.

Changes to potential benefit duration have somewhat larger effects for uncovered job seekers, especially on the share city stayers and unemployment.

We would like to stress three insights from these simulations. First, studying average wages and average commuting distances will not necessarily provide information on the underlying decisions. Wages and commuting distances move less than reservation wages and search radius. Second, studying how many job seekers work in the same workplace as prior to unemployment is potentially revealing about the allocation of effort. The "share city stayers" reacts strongly to changes in potential benefit duration but not at all to changes in the benefit level with corresponding changes in search effort. Third, benefit levels affect outcomes less strongly than corresponding changes in the duration of unemployment benefits. This is interesting and somewhat counter-intuitive since changes to the unemployment benefit duration have no immediate impacts on covered job seekers. But job seekers are forward looking and the threat of loosing benefit payments changes decisions already well ahead. This finding, based on simulations, is in line with empirical studies on the effects of potential benefit duration vs benefit duration, e.g. Lalive et al. (2006).

Figure 3.15: Policy Effects on Search Strategies from Extended Theory



3.6 Summary and Conclusion

Taking the wage-commute distance arbitrage seriously, the paper has developed and then enriched a search model where the unemployed choose a range over which to search, an intensity of search in that area and how to allocate search effort in a particular city (their previous workplace). This model allows us to define the main concepts and to discipline the empirical analysis. It additionally clarifies the efficiency role for unemployment insurance, namely to alleviate the liquidity constraints of the unemployed. Indeed, if search in space is costly not only in terms of effort but also financially, especially away from the city of residence, benefits will help expanding the range of search under the existence of liquidity constraints.

The data analysis uncovers many regularities. Based on an administrative social security dataset covering all newly unemployed workers in Austria, which contains information on the current residence, the previous workplace and the subsequent workplace for those re-hired, we established a set of facts.

A. Commute time is dispersed and leads to a wage-distance trade-off: i) in a sample of employed workers having entered the unemployment spells, 57.2% of them had more than 20 minutes of one way commute distance, while 23.7% had more than 40 min to the workplace; 22.6% of them used to work in the same city as where they live; ii) there is a positive correlation in the data between finding a job with a higher wage and finding a job with a higher distance; iii) almost as many people face a wage increase as a wage decrease after finding a new job; iv) almost as many workers face a commute distance increase as people facing a commute distance decrease.

B. Reservation wage strategies vary over time: the hazard rate of getting a lower paid job increases relative to the hazard rate of getting a better paid jobs, both for individuals facing an increase in the commute distance and for individuals facing a decline in the commute distance.

C. Spatial search strategies vary over time too: i) over time, after the initial peak, people are much less likely to find a job in the same city than to face an increase in the commute distance. An interpretation is that job seekers initially search more intensely in the same city and then prospect relatively more outside the city; those prospecting at a shorter distance may be liquidity constrained unemployed, who will accept a lower wage but cannot afford expensive job search; ii) over time, the likelihood to commute longer distances increases relative to other hazard rates (no distance change or lower distance); an interpretation is that, for a given wage offer, the reservation distance increases over time for those not liquidity constrained.

D. Disincentive effects of social transfers (UI, UA and duration of UI) are

quite robust: i) they imply a negative effect on hazard rates and this applies to all sub-hazard rates (higher and lower wages, higher and lower commute distance); ii) in relative terms however, they raise the incidence of getting higher paid jobs as compared to the previous wage; iii) Quantitatively, our calibrated model implies an elasticity of hazard rate to benefits of -0.12; while explorations of the role of strict liquidity constraints suggest negligible effects on unemployment.

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3.7 Appendix

A Theory Appendix

A.1 Proof of Lemma 1

Proof. The proof is easy. The reservation wage $R(\rho)$ is defined by

$$\begin{aligned} r_c W(R(\rho),\rho) &= R(\rho) - c(\tau\rho) + s(U_c - W(R(\rho),\rho)) \\ &= R(\rho) - c(\tau\rho) + s(U_c - U) = r_c U \end{aligned}$$

so that

$$R(\rho) = \tau\rho + r_c U + s(U - U_c) \tag{3.8}$$

and similarly,

$$\begin{aligned} r_c W(R_c(\rho),\rho) &= R_c(\rho) - c(\tau\rho) + s(U_c - W(R_c(\rho),\rho)) \\ &= R_c(\rho) - c(\tau\rho) = r_c U_c \end{aligned}$$

so that

$$R_c(\rho) = c(\tau\rho) + r_c U_c \tag{3.9}$$

Hence Lemma 1.

A.2 Extended Model: Two Levels of Unemployment Compensation

This Section contains the equations from the extended model presented in Section 3.4. As in Section 3.2, we use notations $F_\rho(w)$ and $G(\rho)$ for the cumulated distributions of wages and distances separately. The Bellman equations with two levels of unemployment insurance are, respectively:

$$\begin{aligned} r_c U_c(D) = B - c(D_c) + \delta_c D_c B \\ + 2\pi\lambda_c \int_0^{D_c} \left(\int_w Max[W(w,\rho) - U_c; 0] dF_\rho(w) \right) dG(\rho) + \alpha(U - U_c) \end{aligned} \tag{3.10}$$

$$rU(D) = b - c(D) + \delta D b + 2\pi\lambda \int_0^{D} \left(\int_w Max[W(w,\rho) - U; 0] dF_\rho(w) \right) dG(\rho) \tag{3.11}$$

The value functions for employment are, under the assumption that the employed workers have the same easy access to credit and saving plans as the covered unemployed:

$$r_c W_c(w, \rho) = w - c(\tau\rho) + \delta_c(\tau\rho)w + s(U_c - W_c(w, \rho)) \tag{3.12}$$

where $c(\tau\rho)$ is the commute cost for employees. The presence of τ captures the possibility that search and commuting distance may affect disutility differently. Equation 3.12 similarly applies to uncovered workers.

The solutions proceed from the previous analysis, except that $\frac{\partial W_c}{\partial w}(w, \rho) = \frac{1}{r_c+s}(1 + \delta_c\tau\rho)$; $\frac{\partial W_c}{\partial \rho}(w, \rho) = \frac{1}{r_c+s}(-c'(\tau\rho)\tau + \delta_c\tau w)$ so that, denoting by $R_c(\rho)$ the reservation wage of an eligible worker associated with distance ρ, defined as $W_c(R_c(\rho), \rho) = U_c(D_c^*) = U_c$ (for simplicity we drop the optimal strategy D_c^*) and by $R(\rho)$ the reservation wage of an uncovered worker associated with distance ρ, defined as $W(R(\rho), \rho) = U(D^*) = U$, we can rewrite the value of employment as a linear function of w:

$$W_c(w, \rho) - U_c = \frac{1 + \delta_c\tau\rho}{r_c + s}(w - R_c(\rho)) = S_c(w, \rho) \tag{3.13}$$

Similar steps lead to

$$W(w, \rho) - U = \frac{1 + \delta\tau\rho}{r + s}(w - R(\rho)) = S(w, \rho)$$

We can now derive the reservation wages:

$$R(\rho) = \frac{1}{1 + \delta\tau\rho}[c(\tau\rho) + r_c U + s(U - U_c)] \tag{3.14}$$

$$R_c(\rho) = \frac{1}{1 + \delta_c\tau\rho}[c(\tau\rho) + r_c U_c] \tag{3.15}$$

From 3.14 and 3.15 we can compute the derivative of the reservation wage with respect to distance:

$$\frac{\partial R}{\partial \rho} = \frac{c'(\tau\rho)}{1 + \delta\tau\rho} - \frac{\delta\tau}{(1 + \delta\tau\rho)^2}[c(\tau\rho) + r_c U + s(U - U_c)] \tag{3.16}$$

Equation 3.16 shows that the reservation wage is a non linear function of commute distance. This slope should be compared to the slope of the empirical relationship displayed in the last panel of Figure 3.5. Our calibration ensures the positivity of the relationship.

Optimal Search Strategies

The first order condition on the radius can now be derived. Let $w^{\max}$ be the upper support of the wage distribution. We have

$$rU(D) = b - C(D) + \delta Db + 2\pi\lambda\mathbb{E}_{w,\rho}S(w,\rho) \tag{3.17}$$

$U(D)$ is maximised when

$$C'_D(D^*) - \delta b = 2\pi\lambda\mathbb{E}_w S(w, D^*)g(D^*) \tag{3.18}$$

Similarly, $U_c(D_c, \lambda_c)$ is maximised when:

$$C'_D(D_c^*) - \delta_c B = 2\pi\lambda_c\mathbb{E}_w S(w, D_c^*)g(D_c^*) \tag{3.19}$$

Similar expression as in Section 3.2 hold for the optimal search intensity λ and λ_c.

Extension of Lemma 3 to Two Types of Unemployed Workers

We now have:

$$rU(D,\lambda) = b - C(D,\lambda) + 2\pi\lambda\int_0^D\int_{R(\rho)}^{w^{\max}} S(w,\rho)dF_\rho(w)dG(\rho)$$
$$r_cU_c(D_c,\lambda_c) = B - C(D_c,\lambda_c) + 2\pi\lambda_c\int_0^{D_c}\int_{R_c(\rho)}^{w^{\max}} S(w,\rho)dF_\rho(w)dG(\rho) + \alpha(U - U_c)$$

The first value equation, through the envelope condition, leads as before to: $r\frac{dU}{db} = 1$; the second value equation leads to

$$(r_c + \alpha)\frac{dU_c}{dB} = 1 + \alpha\frac{dU}{dB}$$

Dynamics of the Pool of Covered and Uncovered Job Seekers

Let us denote by $N_c(t)$ and $N_{nc}(t)$ the number of covered and uncovered unemployed workers at time t for a given cohort entering unemployment at time $t = 0$. We have, for all $t > 0$:

$$dN_c/dt = -(haz_c + \alpha)N_c$$
$$dN_{nc}/dt = -hazN_{nc} + \alpha N_c$$

These first order partial differential equations are easy to solve. In particular, we have that:

$$N_c(t) = N_c(0)e^{-(haz_c+\alpha)t} \tag{3.20}$$

$$N_{nc}(t) = N_{nc}(0)e^{-haz.t} + \frac{\alpha e^{-haz.t}}{haz_c + \alpha - haz} N_c(0)\left(1 - e^{-(haz_c+\alpha-haz)t}\right) \tag{3.21}$$

where both lines are obtained in fixing the integration constant to get the initial value at time $t = 0$ (entrance into the unemployment spell). Further, if all new entrants are covered, we have that $N_{nc}(0) = 0$. The two equations (3.20) and (3.21) determine the fractions of each of the four groups, that is, the covered and uncovered job seekers in the population of applicants.

A.3 Extended Model: Directing Search Towards the Previous City

The new first order conditions on optimal search intensity now read as follows:

$$\begin{aligned} C'_\lambda(D, \lambda, \lambda^0) = 2\pi \Bigg[& \int_0^{d_{0^-}} \int_{R(\rho)}^{w^{\max}} \left[\frac{1 + \delta\tau\rho}{r + s}(w - R(\rho))\right] dF_\rho(w) dG(\rho) \\ & + \int_{d_{0^-}}^{D} \int_{R(\rho)}^{w^{\max}} \left[\frac{1 + \delta\tau\rho}{r + s}(w - R(\rho))\right] dF_\rho(w) dG(\rho) \Bigg] \end{aligned} \tag{3.22}$$

$$C'_{\lambda^0}(D, \lambda, \lambda^0) = 2\pi \int_{d_{0^-}}^{d_{0^+}} \int_{R(\rho)}^{w^{\max}} \left[\frac{1 + \delta\tau\rho}{r + s}(w - R(\rho))\right] dF_\rho(w) dG(\rho) \tag{3.23}$$

A.4 Extended Model: Hazard Rates

There are now six sub-hazard rates $sub{-}haz(w^+, d^+)$, $sub{-}haz(w^+, d^-)$, $sub{-}haz(w^-, d^+)$, $sub - haz(w^-, d^-)$, $sub - haz(w^+, d_0)$, $sub - haz(w^-, d_0)$, where the sum of these six sub-hazard rates is the total hazard rate haz. Taking advantage of the empirical evidence, we assign a peculiar role to the previous workplace, here proxied by the median distance. To discretize space, we define the area around the previous workplace as a small circle centered in d_0. Let ε be the radius of this small circle, it is useful to define $d_{0^-} \equiv d_0 - \varepsilon$ and $d_{0^+} \equiv d_0 + \varepsilon$. We calibrate ε to be 10% of d_0.

Moreover, we allow for the possibility that individuals exert effort in the previous workplace at a different (possibly higher) rate, denoted by λ^0 and λ^0_c for uncovered and covered workers, respectively. Notice that, under this assumption, the value of unemployment should be rewritten:

$$rU(D) = b - c(D) + \delta Db + 2\pi\lambda^0 \mathbb{E}_\rho|_{\rho\in[d_{0-},d_{0+}]} S(w,\rho) + 2\pi\lambda\mathbb{E}_\rho|_{\rho\in[0,d_{0-})\cup(d_{0+},D]} S(w,\rho)$$

The optimality conditions conversely do not change, provided that we always ensure $D > d_{0+}$ and $D_c > d_{0+,c}$.

We thus define the total hazard rate of covered and uncovered workers as:

$$\begin{aligned}
haz &= 2\pi\lambda\left[\int_0^{d_{0-}}\int_{R(\rho)}^{w^{\max}} dF_\rho(w)dG(\rho) + \int_{d_{0+}}^{D}\int_{R(\rho)}^{w^{\max}} dF_\rho(w)dG(\rho)\right] \\
&+ 2\pi\lambda^0\left[\int_{d_{0-}}^{d_{0+}}\int_{R(\rho)}^{w^{\max}} dF_\rho(w)dG(\rho)\right] \\
&= 2\pi\lambda\left[\int_0^{d_{0-}}[1-F_\rho(R(\rho))]dG(\rho) + \int_{d_{0+}}^{D}[1-F_\rho(R(\rho))]dG(\rho)\right] \\
&+ 2\pi\lambda^0\left[\int_{d_{0-}}^{d_{0+}}[1-F_\rho(R(\rho))]dG(\rho)\right] \\
haz_c &= 2\pi\lambda_c\left[\int_0^{d_{0-}}\int_{R_c(\rho)}^{w^{\max}} dF_{c,\rho}(w)dG_c(\rho) + \int_{d_{0+}}^{D_c}\int_{R_c(\rho)}^{w^{\max}} dF_{c,\rho}(w)dG_c(\rho)\right] \\
&+ 2\pi\lambda_c^0\left[\int_{d_{0-}}^{d_{0+}}\int_{R_c(\rho)}^{w^{\max}} dF_{c,\rho}(w)dG_c(\rho)\right] \\
&= 2\pi\lambda_c\left[\int_0^{d_{0-}}[1-F_{c,\rho}(R_c(\rho))]dG_c(\rho) + \int_{d_{0+}}^{D_c}[1-F_{c,\rho}(R_c(\rho))]dG_c(\rho)\right] \\
&+ 2\pi\lambda_c^0\left[\int_{d_{0-}}^{d_{0+}}\int_{R_c(\rho)}^{w^{\max}} dF_{c,\rho}(w)dG_c(\rho)\right]
\end{aligned}$$

A.5 Calibration

The calibration strategy is as follows. First, we fix the parameters for which we have some information. For instance, we set B (the unemployment insurance) and b (unemployment benefits) to be 35 % and 3% of the average wage, respectively. Our benchmark calibration assumes an annual interest rate of 4% for workers covered by unemployment insurance (B), while long-term unemployed face a higher borrowing rate ($r_c = 12\%$ annually). We assume wages and distances are distributed log-normally and we set the mean and the standard deviation to their empirical counterparts. We can allow for arbitrary values of correlation, but in the baseline calibration strategy we start with independent distributions. We set the separation rate so as to match an average unemployment rate around 6%. For the disutility component of the search cost function we assume separability between distance and search intensity and convexity in each argument ($\eta_c = \eta_\lambda = 1.5$). Importantly, we assume that agents (both covered and uncovered) weight less the effort provided to search in the previous workplace rather than outside ($\gamma^{\lambda^0}, \gamma_c^{\lambda^0} < 1$). Moreover, covered workers suffer less from the intensity of search in the previous workplace than uncovered agents ($\gamma_c^{\lambda^0} < \gamma^{\lambda^0}$). This is an important assumption: because we do not introduce other dimensions of heterogeneity, the asymmetry in the search cost is needed to rationalize the empirical observations that covered workers exit unemployment more quickly and they are relatively more "city stayers". The weight on the cost of search intensity has an alternative interpretation as the efficiency of the search process. It is rational to make the assumption that covered workers are relatively more efficient in searching jobs for several not self-excluding reasons, as discussed in Section 3.5.1. We set $\delta = \delta_c = -0.2$; this calibration implies that, for any given income (consumption) level, agents are better off when they search/commute less.

Given that our dataset does not provide any specific information about the private cost of commuting and the transport infrastructures, we choose a linear commuting cost function with coefficient (τ) equal to 1. This monetary component also enters the search cost function with the same coefficient.

For a given set of parameters, the dynamic of the hazard rate, the sub-hazards and their ratios is driven by the relative share of workers belonging to the covered or uncovered state, respectively. More precisely, in each period the hazard rate is a weighted average of the hazard rate of covered and uncovered workers, where the weights are represented by the share of workers in these two states, respectively. As time goes, the share of uncovered workers increases, thus triggering the dynamic of the hazard rates. Hence, in the model, the dynamic is entirely due to the different search strategies chosen by covered and uncovered workers.

Table 3.8: Effects of Potential Benefit Duration on Covered Job Seekers

PBD (weeks)	**0**	**26**	**52**	**99**	**104**
Observed outcomes					
Average wage (euros per day)	61.73	65.15	66.24	67.21	67.27
Average distance (min)	27.51	25.14	23.97	22.53	22.41
Hazard rate	0.2307	0.1004	0.0707	0.0481	0.0466
Rejection rate	0.004	0.013	0.015	0.017	0.017
Share city stayers	0.15	20.54	24.67	28.38	28.67
Unemployment	0.000	0.015	0.025	0.042	0.043
Decisions					
Reservation wage	33.55	44.80	48.41	51.83	52.09
Search radius	1.52	1.14	1.01	0.87	0.86
Effort outside d_0	0.0297	0.0144	0.0108	0.0079	0.0077
Effort inside d_0	0.1042	0.0520	0.0401	0.0307	0.0301

B Supplementary Results: Theory

B.1 Policy Simulations

Tables from 3.8 to 3.13 report the changes in the observed outcomes and the decisions of the unemployed for different policy experiments. Regarding the Potential Benefit Duration, we consider the absence of UA (0 weeks), 26 weeks like in the US, the double of this value (52 weeks) and two extreme values (99-104 weeks) which correspond to the maximum reached during the last recession. For the UI we consider a wide range of values, varying the replacement rate from 0 to 80%, which is among the maximum values observed in reality (Denmark). In Austria the current replacement rate is around 40%. UA is expressed in terms of UI, ranging from 0 to 50%.

The information conveyed by the tables are the same as in Figure 3.15 and discussed in Section 3.5.4.

B.2 The Strict Liquidity Constraints Case

In the case the unemployed have decumulated their assets and face a subsistence level for consumption, say $\underline{\mathcal{C}}$, they face the following strong cash constraint that prevents them from searching optimally in space:

$$b \geq \underline{\mathcal{C}} + M(D)$$

Table 3.9: Effects of Potential Benefit Duration on Uncovered Job Seekers

PBD (weeks)	**0**	**26**	**52**	**99**	**104**
Observed outcomes					
Average wage (euros per day)	60.51	60.34	60.29	60.25	60.24
Average distance (min)	25.70	25.86	25.91	25.95	25.96
Hazard rate	0.0446	0.0469	0.0477	0.0484	0.0484
Rejection rate	0.003	0.003	0.003	0.003	0.003
Share city stayers	11.58	4.61	3.46	2.70	2.65
Unemployment	0.082	0.048	0.041	0.035	0.034
Decisions					
Reservation wage	34.00	33.19	32.94	32.70	32.68
Search radius	1.21	1.23	1.24	1.25	1.25
Effort outside d_0	0.0078	0.0082	0.0083	0.0084	0.0084
Effort inside d_0	0.0072	0.0075	0.0076	0.0077	0.0077

Table 3.10: Effects of Unemployment Insurance (B) on Covered Job Seekers

Replacement rate	**0**	**0.20**	**0.4**	**0.6**	**0.8**
Observed outcomes					
Average wage (euros per day)	65.34	64.97	64.69	64.47	64.31
Average distance (min)	26.35	25.86	25.39	24.92	24.44
Hazard rate	0.1209	0.1162	0.1114	0.1066	0.1016
Rejection rate	0.011	0.012	0.012	0.012	0.012
Share city stayers	18.99	18.93	18.86	18.80	18.72
Unemployment	0.012	0.012	0.012	0.012	0.012
Decisions					
Reservation wage	43.38	43.72	44.06	44.42	44.79
Search radius	1.31	1.23	1.17	1.11	1.06
Effort outside d_0	0.0169	0.0163	0.0157	0.0151	0.0145
Effort inside d_0	0.0592	0.0579	0.0565	0.0552	0.0538

Table 3.11: Effects of Unemployment Insurance (B) on Uncovered Job Seekers

Replacement rate	**0**	**0.20**	**0.4**	**0.6**	**0.8**
Observed outcomes					
Average wage (euros per day)	60.37	60.37	60.36	60.36	60.35
Average distance (min)	25.83	25.84	25.84	25.85	25.85
Hazard rate	0.0466	0.0466	0.0467	0.0468	0.0468
Rejection rate	0.003	0.003	0.003	0.003	0.003
Share city stayers	4.93	5.03	5.15	5.27	5.40
Unemployment	0.050	0.051	0.052	0.052	0.053
Decisions					
Reservation wage	33.47	33.45	33.42	33.40	33.37
Search radius	1.23	1.23	1.23	1.23	1.23
Effort outside d_0	0.0081	0.0081	0.0081	0.0082	0.0082
Effort inside d_0	0.0074	0.0074	0.0074	0.0075	0.0075

Table 3.12: Effects of Unemployment Assistance (b) on Covered Job Seekers

Percentage of B	**0**	**0.25**	**0.5**
Observed outcomes			
Average wage (euros per day)	64.69	64.88	65.09
Average distance (min)	25.55	25.41	25.23
Hazard rate	0.1145	0.1089	0.1028
Rejection rate	0.012	0.012	0.013
Share city stayers	18.81	19.03	19.30
Unemployment	0.012	0.012	0.012
Decisions			
Reservation wage	43.46	44.04	44.70
Search radius	1.19	1.17	1.15
Effort outside d_0	0.0161	0.0154	0.0147
Effort inside d_0	0.0576	0.0554	0.0529

Table 3.13: Effects of Unemployment Assistance (b) on Uncovered Job Seekers

Percentage of B	**0**	**0.25**	**0.5**
Observed outcomes			
Average wage (euros per day)	60.31	60.47	60.66
Average distance (min)	26.00	25.52	24.97
Hazard rate	0.0481	0.0439	0.0395
Rejection rate	0.003	0.003	0.004
Share city stayers	5.10	5.16	5.21
Unemployment	0.050	0.055	0.062
Decisions			
Reservation wage	32.53	33.76	35.12
Search radius	1.25	1.19	1.12
Effort outside d_0	0.0083	0.0077	0.0070
Effort inside d_0	0.0076	0.0071	0.0066

Lemma 5 (**strict liquidity constraints**). *In the absence of assets and under separability of the cost function, e.g. $C(D, \lambda) = M(D) + e(\lambda, D)$ where the first part is monetary, the constrained range of search is sub-optimal if*

$$\bar{D}(b) = M^{-1}(b - \underline{\mathcal{C}}) < D^*$$

The constrained value is increasing in the level of benefits and decreasing in the subsistence level. In turn, the optimal effort λ^* will itself react to the constrained value $\bar{D}(b)$.

This Lemma introduces a new role of unemployment insurance in the presence of imperfect financial markets as studied in Baily (1978), Chetty (2008) or Werning (2002) or Shimer and Werning (2003). It recognizes that search costs are not only time costs or disutility costs, but have a monetary component due to the existence of the spatial dispersion of jobs. The equivalent results of Lemma 3 in the strict liquidity constraints case can be summarized as follows:

Lemma 6 (**unemployment benefits impact**). *Under strict liquidity constraints as in Lemma 5, the impact of benefits on U is larger than the inverse of the discount rate.*

The proof of this Lemma and 3 in the text is based on the derivatives of

$$rU(D, \lambda) = b - C(D, \lambda) + 2\pi\lambda \int_0^D \int_{R(\rho)}^{w^{\max}} S(w, \rho) dF_\rho(w) dG(\rho)$$

with respect to b for the ongoing rate of interest. Denote by $\tilde{D}$ the minimum between the optimal search radius D^* and the constrained level $\bar{D}(b)$: we have

$$\begin{aligned} r\frac{dU}{db} &= 1 \\ &+ \frac{\partial \lambda^*}{\partial b}\left[-C'_\lambda + 2\pi \mathbb{E}_{w,\rho} S(w,\rho)\right] \\ &+ \frac{\partial \widetilde{D}}{\partial b}\left[-C'_D(\widetilde{D},\lambda) + 2\pi\lambda \mathbb{E}_w S(w,\widetilde{D})\right] \\ &+ 2\pi\lambda \int_0^D \frac{\partial R(\rho)}{\partial b}\left[-S(R(\rho),\rho) f(\rho)\right] dG(\rho) \end{aligned}$$

The last line is by definition equal to zero since the surplus is equal to zero at $R(\rho)$. In interior solutions, by the envelope theorem, the second and third lines are equal to zero as well. Hence, the effect of benefits is equivalent to a permanent rise in the income of the unemployed workers, who will enjoy both higher benefits as unemployed and choose higher wages in the future. The situation is different for credit constrained unemployed workers; indeed, if $D = \bar{D}(b) < D^*$ is the constrained level of the range of search, then the envelope condition of the third line does not hold. In this case, $-C'_D + 2\pi\lambda\mathbb{E}_w S(w, D^*) > 0$; then, the effect of benefits on the value of unemployment is larger than $1/r$.

Figures 3.16 and 3.17 compare the dynamics of the simulated hazard rates and of the search strategies with and without liquidity constraints.

Starred lines in Figures 3.18 and 3.19 represent policy simulations under a calibration that implies strict liquidity constraints for uncovered workers $(D = D\bar{(b)})$. Covered workers turn out not to be constrained because the unemployment insurance they are entitled to is substantially higher than assistance. The results are especially interesting for policy changes affecting unemployment assistance (b). For low values of b, uncovered agents are liquidity constrained: this implies a sub-optimally low search radius and hazard rate. Notice that the presence of liquidity constraints affects search strategies also at early stages of the unemployment spell, since agents take into account the possibility of switching to the uncovered state.

Table 3.14 summarizes these results in terms of elasticities.

Figure 3.16: Simulated Hazard Rates: Strict Liquidity Constraints for the Unemployed Under the UA Regime

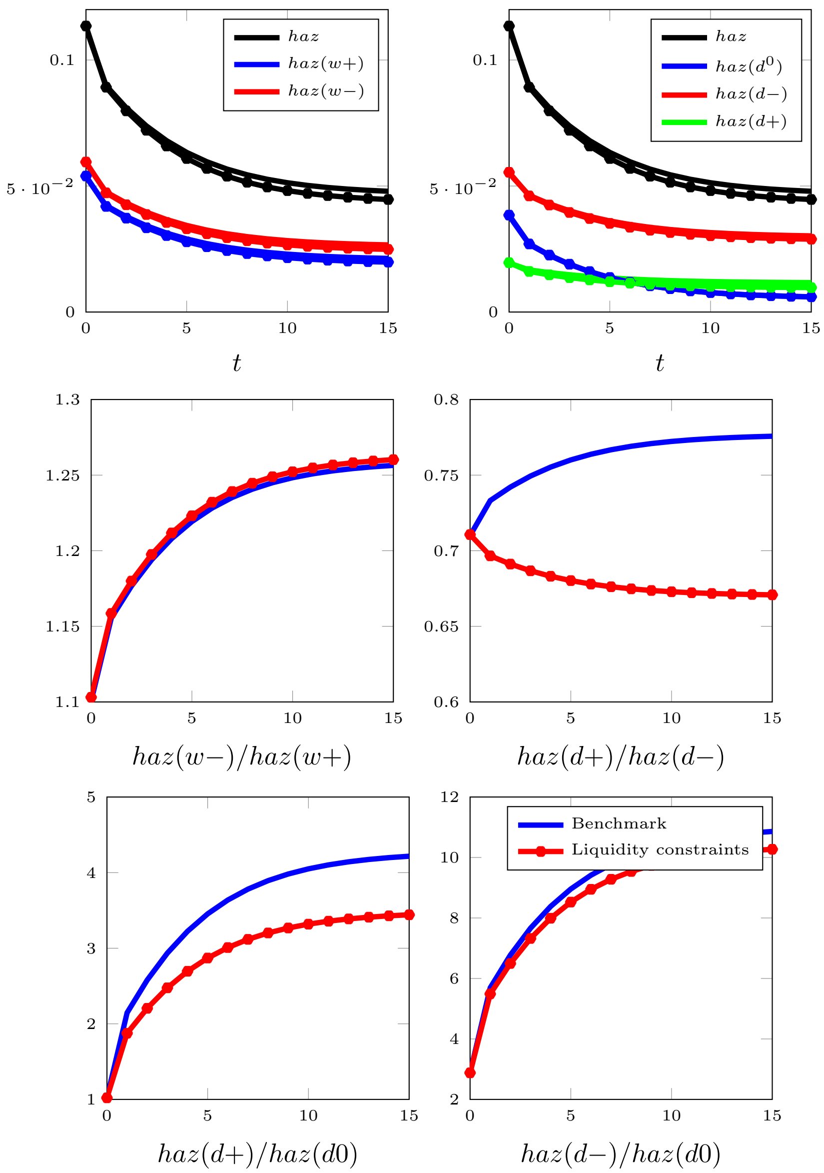


Figure 3.17: Search Strategies: Strict Liquidity Constraints for the Unemployed Under the UA Regime

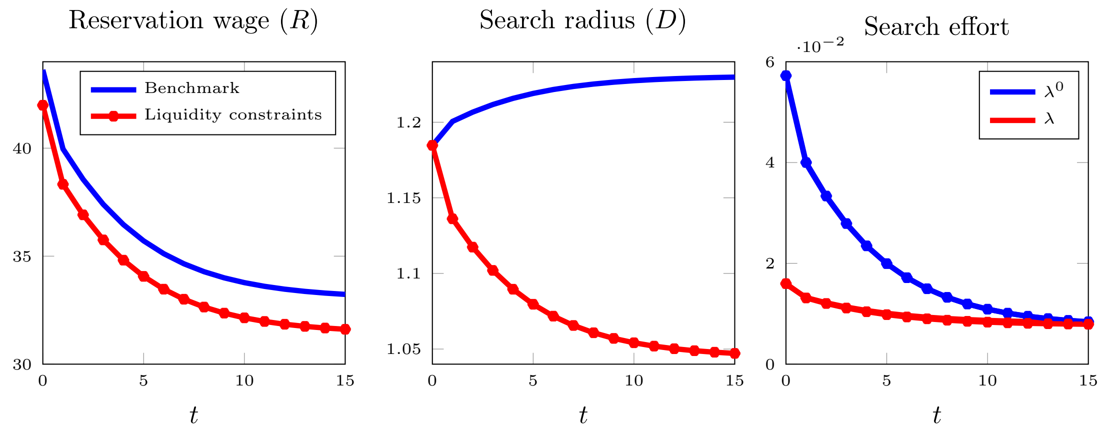
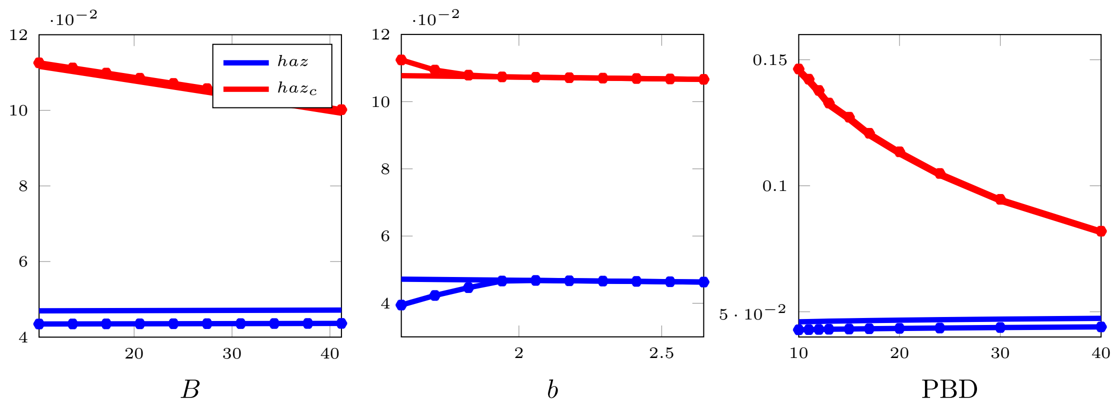


Figure 3.18: Policy Effects on Hazard Rates: Strict Liquidity Constraints for the Unemployed Under the UA Regime



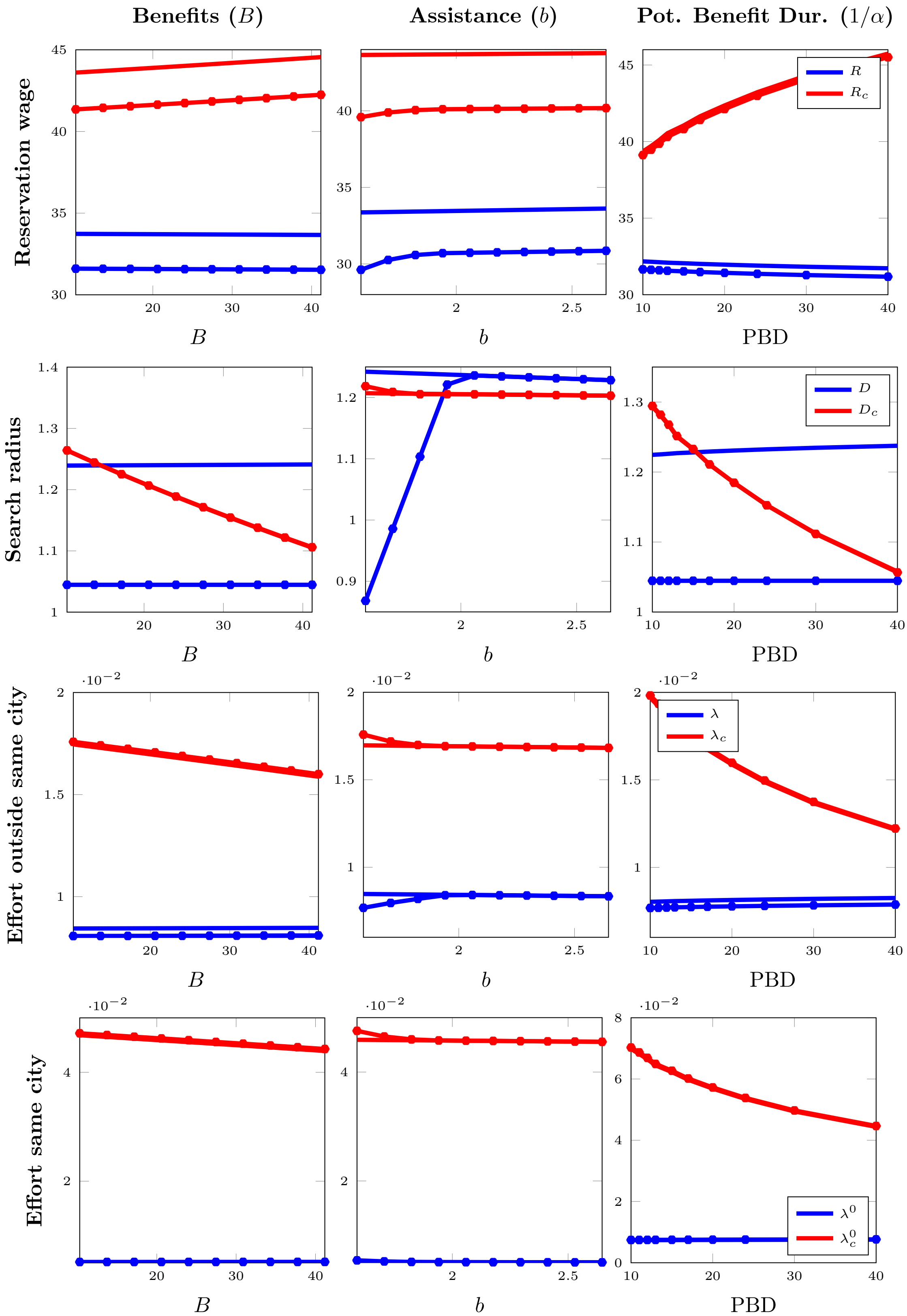
Notes: solid lines refer to the regime with mild liquidity constraints ($r > r_c$); dotted lines show results when unemployed agents under UA are subject to strict liquidity constraints.

Table 3.14: Elasticities of the Model With Strict Liquidity Constraints (for Workers Under the UA Regime)

	Covered job seekers			**Uncovered job seekers**		
	B	b	PBD	B	b	PBD
Observed outcomes						
Average wage	-0.00701	0.00934	0.02091	-0.00018	0.05378	-0.00115
Average distance	-0.03702	-0.00802	-0.05842	0.00000	0.46273	0.00000
Hazard rate	-0.08625	-0.11424	-0.40725	0.00277	0.53952	0.01776
Rejection rate	0.02079	0.10929	0.24895	-0.01042	1.56581	-0.06749
Share city stayers	-0.00558	-0.19833	0.23877	0.04798	-0.37945	-0.35029
Unemployment	0.02930	0.06746	0.75717	0.02651	-0.41595	-0.20347
Decisions						
Reservation wage	0.01597	0.03255	0.09952	-0.00160	0.09303	-0.01034
Search radius	-0.10181	-0.02287	-0.15013	0.00000	1.29386	0.00000
Effort outside d_0	-0.07337	-0.09554	-0.33739	0.00262	0.31476	0.01683
Effort inside d_0	-0.04732	-0.09486	-0.31042	0.00277	-0.15647	0.01779

Notes: The elasticity of y with respect to x is defined as $\left(\frac{\Delta y}{y}\right) / \left(\frac{\Delta x}{x}\right)$. Elasticities are computed from simulations, considering the following policy changes: ΔB from 40 to 50 % of the previous wage; Δb from 8% to 10% of the benchmark B; Δ PBD 30 to 39 weeks.

Figure 3.19: Policy Effects on Search Strategies: Strict Liquidity Constraints for Unemployed Under the UA Regime



Notes: solid lines refer to the regime with mild liquidity constraints ($r > r_c$); dotted lines show results when unemployed agents under UA are subject to strict liquidity constraints.

B.3 Maximizing the Social Welfare Function with Benefits

The social welfare function is the sum of the value of unemployment and the social value of employment, possibly incorporating the social costs of commutes, to which unemployment insurance and assistance must be deducted. Some intermediate results will be useful. Denote by u_c and u_{nc} the number of unemployed workers who are covered and non covered, respectively; we have the differente rates of unemployment by equality of inflows and outflows:

$$s(1 - u_c - u_{nc}) = u_c.(hazard_c + \alpha)$$
$$u_c\alpha = u_{nc}.hazard_{nc}$$

$$\begin{aligned} u_c &= \frac{s}{\alpha + s + hazard_c + \alpha s/hazard_{nc}}; \\ u_{nc} &= \frac{s}{\alpha + s + hazard_c + \alpha s/hazard_{nc}} \frac{\alpha}{hazard_{nc}} \\ u &= u_c + u_{nc} \end{aligned}$$

There are two special cases: when $\alpha = 0$ we obtain $u = s/(s + hazard_c)$; and when $hazard_c = hazard_{nc}$, we also have $u = s/(s + hazard_c)$.

Introducing the notations:

$$r\tilde{U}(D, \lambda) = 0 \times b - C(D, \lambda) + 2\pi\lambda \int_0^D \int_{R(\rho)}^{w^{\max}} \tilde{S}(w, \rho) dF_\rho(w) dG(\rho)$$
$$r_c\tilde{U}_c(D_c, \lambda_c) = 0 \times B - C(D_c, \lambda_c) + 2\pi\lambda \int_0^D \int_{R(\rho)}^{w^{\max}} \tilde{S}(w, \rho) dF_\rho(w) dG(\rho) + \alpha(\tilde{U} - \tilde{U}_c)$$

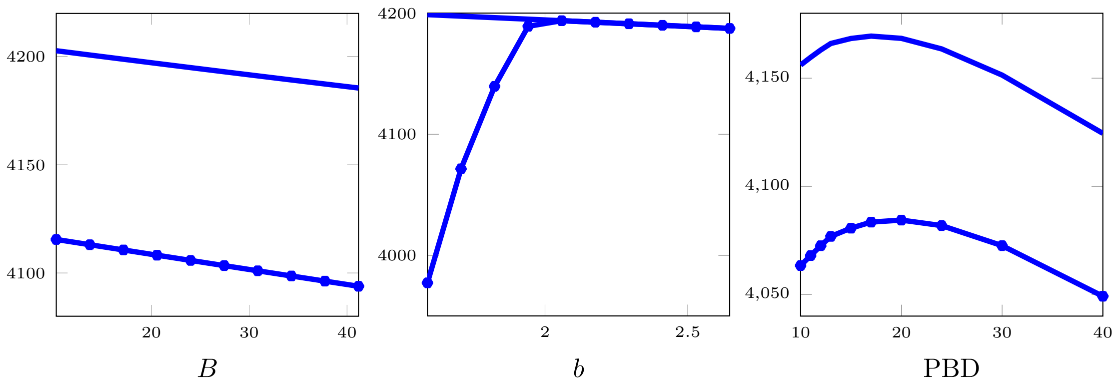
the social welfare function is therefore

$$\Omega = u_{nc}\tilde{U}(D, \lambda) + u_c\tilde{U}(D_c, \lambda_c) + (1 - u_c - u_{nc})\mathbb{E}_{w,\rho} \left[W(w, \rho) - SC(\rho) \right].$$

where $SC(\rho)$ represent the social costs of commuting[15]. We vary B and b under two polar cases: one where agents under unemployment assistance (b) are only mildly constrained; one where agents under unemployment insurance B are not liquidity constrained but agents under unemployment assistance b cannot afford to pay for long search distances. The effects of policy changes on social welfare are plotted in Figure 3.20 , where the solid line represents the behavior of the social welfare function under mild financial

[15] For the social costs of commuting we utilize the following specification: $SC(\rho) = \tau^{social} \left[\frac{haz \cdot u_{nc}}{s} \mathbb{E}_{nc}(\rho) + \frac{haz_c \cdot u_c}{s} \mathbb{E}_c(\rho) \right]$.

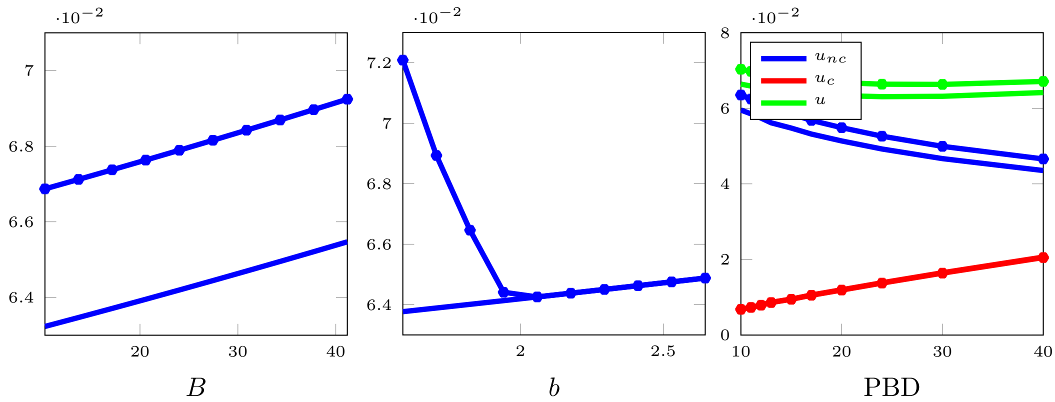
Figure 3.20: Policy Effects on Social Welfare



constraints and the starred line refers to the case where the uncovered workers are liquidity constrained for low values of assistance. It can be seen that the socially optimal level of unemployment insurance is zero, since welfare declines monotonically with B. Instead, if under mild liquidity constraint the same is true for b (unemployment assistance), in the more realistic case of liquidity constraints for households in the assistance regime, there is an optimal level of unemployment assistance and social welfare, which first goes up as the range of search can be extended and the constraints are reduced. Once the cash constraint is suppressed however, higher levels of assistance reduce search intensity and welfare goes down again.

The gap between the dotted line and the solid line in Figure 3.21 represents the percentage points of unemployment that can be attributed to the existence of strict liquidity constraints, the fact that the unemployed cannot search over the optimal range. This gap is 0.3 percentage points in the left panel, but the gap depends very much on the value of b which determines the value of D in the case of strict liquidity constraints; in the middle panel, the difference is as high as 0.072-0.064, that is 0.8 percentage points of unemployment.

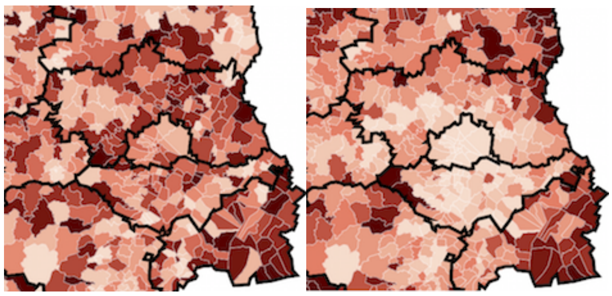
Figure 3.21: Policy Effects on the Unemployment Rate: Strict Liquidity Constraints for Unemployed Under the UA Regime



Notes: solid lines refer to the regime with mild liquidity constraints ($r > r_c$); dotted lines show results when unemployed agents under UA are subject to strict liquidity constraints.

C Supplementary Results: Empirics

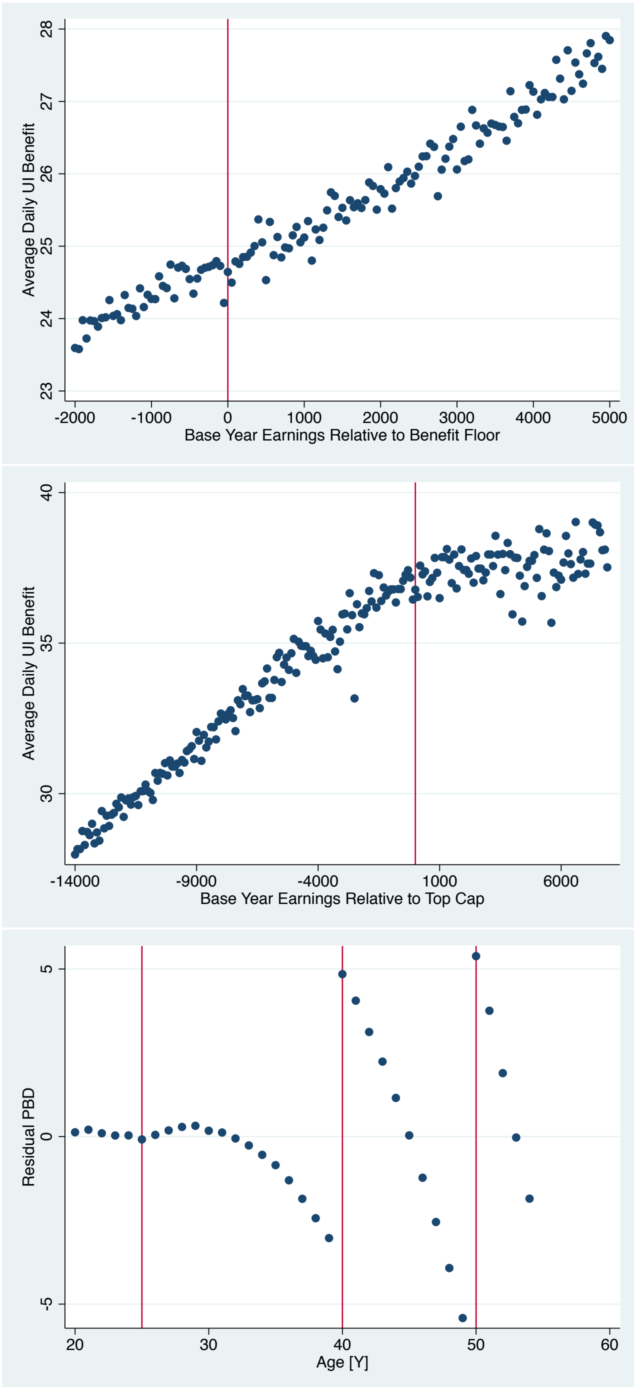
Figure 3.22: Average Commuting Time by Workplace (left) and Residency (right) around Vienna



Notes: The figure is a zoomed-in version of Figures 3.3 and 3.4.

Table 3.16 reports the estimates of the effects of the policy parameters when the wage and the distance dimensions are considered jointly. We start by looking into how UI affects the rate of finding a better paying job, closer to home ($w+/d-$). This transition should not be affected by changes in the reservation wage or search radius, just by search intensity, thus representing a convenient baseline. Indeed, point estimates on UI parameters are small in absolute value. Compared to this baseline, UI significantly reduces exits to worse paid jobs, regardless of whether the job is closer or farther from home ($w-/d+$, $w-/d-$). Compared to the baseline, neither UI benefits nor assistance affect exists to better paid jobs located further away from home ($w+/d+$). The key effect on distance is via potential benefit duration which reduces the transitions to jobs located further from home but has no effect on the baseline. Results for job seekers who make transitions into jobs that are different from the previous one suggest reservation wages adjust but search radius does

Figure 3.23: Kinks in the UI Benefit Schedule and Discontinuity in Age



Notes: The top graph shows average daily UI benefits around the bottom kink (vertical line). The middle graph shows the average daily UI benefits around the top kink (vertical line). The bottom graph shows the residual variation from discontinuities in the eligibility rule for PBD at age 40 and 50 conditional on observable individual characteristics.

not.

The remaining bivariate transition rates feature either a wage that stays the same (w_0) or a distance that stays the same (d_0). Results from outcomes where distance stays the same (columns 7 to 9) indicate a strong effect of UI benefits and assistance on transitions to worse paid, $w-/d_0$, (or equally paid, w_0/d_0) jobs, compared to the transition to better paid jobs ($w+/d_0$). The outcomes where wages stay the same (columns 3, 6 and 9 again) show somewhat more reduced transitions to the same city (w_0/d_0) as unemployment benefits or assistance increase, compared to being either further ($w_0/d+$) or closer to home ($w_0/d-$). Results on bivariate estimates are generally consistent with the univariate results.

Table 3.15: Cox-model Estimates, Same Results as Table 3.3 But Full Set of Covariates Shown

	(all)	(all)	(w+)	(w-)	(w0)	(d+)	(d-)	(d0)
B	-0.542***	-0.640***	-0.350***	-2.494***	-1.475***	-0.537***	-0.652***	-0.910***
	(0.031)	(0.026)	(0.036)	(0.054)	(0.063)	(0.037)	(0.040)	(0.060)
b		-0.271***	-0.314***	-0.589***	-0.522***	-0.261***	-0.289***	-0.258***
		(0.033)	(0.052)	(0.044)	(0.100)	(0.047)	(0.052)	(0.082)
PBD [weeks]	-0.003***	-0.001	-0.004**	0.006***	-0.000	-0.004***	-0.000	0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.001)	(0.002)	(0.002)
Time to Next Large City	0.002***	0.002***	0.002***	0.001***	0.003***	0.002***	0.003***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Altitude [100m]	0.036***	0.035***	0.037***	0.044***	0.021***	0.027***	0.015***	0.092***
	(0.003)	(0.003)	(0.004)	(0.004)	(0.007)	(0.004)	(0.005)	(0.006)
Married	0.117***	0.115***	0.128***	0.119***	0.150***	0.105***	0.134***	0.102***
	(0.007)	(0.007)	(0.010)	(0.010)	(0.019)	(0.010)	(0.011)	(0.018)
Numb. Children	-0.009**	-0.008**	-0.014***	0.005	0.004	-0.011**	-0.007	-0.005
	(0.004)	(0.004)	(0.005)	(0.005)	(0.009)	(0.005)	(0.006)	(0.009)
Insured Wage [1000]	-0.069***	-0.070***	-0.098***	-0.001	0.002	-0.074***	-0.070***	-0.056***
	(0.003)	(0.003)	(0.005)	(0.005)	(0.006)	(0.004)	(0.005)	(0.007)
White Collar	-0.187***	-0.187***	-0.025*	-0.304***	-0.147***	-0.251***	-0.153***	-0.111***
	(0.008)	(0.008)	(0.013)	(0.012)	(0.023)	(0.012)	(0.013)	(0.020)
Wage Before ([Euros],w_{-1})	-0.002***	-0.002***	-0.036***	0.003***	-0.007***	-0.003***	-0.001***	-0.005***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)
Age [Y]	0.018**	0.018**	0.008	0.029**	-0.002	0.015	0.016	0.029
	(0.008)	(0.008)	(0.013)	(0.012)	(0.023)	(0.012)	(0.013)	(0.020)
Exp 0-1.99y [Y]	-3.123***	-3.175***	-3.065***	-2.158***	-1.909***	-3.306***	-3.785***	-1.385**
	(0.270)	(0.265)	(0.391)	(0.370)	(0.706)	(0.364)	(0.395)	(0.666)
Exp 2-4.99y [Y]	-0.566***	-0.567***	-0.646***	-0.525***	-0.569***	-0.525***	-0.599***	-0.610***
	(0.060)	(0.060)	(0.080)	(0.085)	(0.153)	(0.081)	(0.087)	(0.132)
Exp 5-9.99y [Y]	-0.119***	-0.117***	-0.096***	-0.116***	-0.082	-0.045*	-0.179***	-0.194***
	(0.019)	(0.019)	(0.027)	(0.028)	(0.052)	(0.026)	(0.030)	(0.045)
Nuts3 FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Squares	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cubic	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spells	154,677	154,677	154,677	154,677	154,677	154,677	154,677	154,677
Individuals	118,343	118,343	118,343	118,343	118,343	118,343	118,343	118,343
Log L	-1613517	-1613357	-666060	-737931	-196332	-748354	-591330	-266454
Share Exits	0.96	0.96	0.40	0.44	0.12	0.44	0.35	0.16

Notes: Duration variable is nonemployment in months. Estimates refer to coefficients. w0 contains changes in wage of +/− 4%. Squares and Cubic refers to the inclusion of polynomials of age, experience and past wage. Voluntary quits and recalls are excluded, only Replacement Rates weakly below 1 and potential benefit durations above 0 are considered. Standard errors are clustered on individual level. Significance is indicated as follows: * (p<0.05), ** (p<0.01), *** (p<0.001)

Table 3.16: Cox-Model Estimates, Sub-Hazards

	$(w+, d+)$	$(w-, d+)$	$(w_0, d+)$	$(w+, d-)$	$(w-, d-)$	$(w_0, d-)$	$(w+, d_0)$	$(w-, d_0)$	(w_0, d_0)
B	-0.305***	-2.489***	-1.341***	-0.287***	-2.553***	-1.447***	-0.618***	-2.380***	-1.795***
	(0.049)	(0.065)	(0.093)	(0.056)	(0.069)	(0.106)	(0.082)	(0.092)	(0.133)
b	-0.316***	-0.625***	-0.354**	-0.306***	-0.604***	-0.619***	-0.312**	-0.433***	-0.845***
	(0.072)	(0.063)	(0.140)	(0.084)	(0.068)	(0.172)	(0.134)	(0.108)	(0.255)
PBD [weeks]	-0.005**	0.004**	-0.007*	-0.004	0.007***	0.002	0.001	0.006*	0.010*
	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)	(0.005)	(0.004)	(0.003)	(0.006)
Nuts3 FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spells	154,677	154,677	154,677	154,677	154,677	154,677	154,677	154,677	154,677
Individuals	118,343	118,343	118,343	118,343	118,343	118,343	118,343	118,343	118,343
Log L	-321561	-333117	-87432	-236532	-282172	-67786	-105012	-119044	-40063
Share Exits	0.19	0.20	0.05	0.14	0.17	0.04	0.06	0.07	0.02

Notes: Duration variable is nonemployment in months. Estimates refer to coefficients. w_0 contains changes in wage of ± 4%. Control Variables: Potential Benefit Duration, net wage used for calculation of replacement rate. Experience in the last two, five and ten years (5 is net of 2 and 10 net of 5 years), altitude of the municipality of residence, time to the next large city, age in years, real wage and occupation of the last job before unemployment, marital status and number of children. Voluntary quits and recalls are excluded, only Replacement Rates weakly below 1 and potential benefit durations above 0 are considered. Standard errors are clustered on individual level. Significance is indicated as follows: * (p<0.1), ** (p<0.05), *** (p<0.01).

D Appendix: Robustness Checks of Cox-Estimates and Some Light Evidence of Strong Credit Constraints

Table 3.17 investigates the robustness of the estimates to the exclusion of largest cities. Differences are marginal.

Table 3.19 is an attempt to decompose the results of the effects of benefits and assistance on different outcomes for individuals likely to be credit-constrained (people with at least 3 years of tenure on the job before UI (getting 2 months of salary as cash-on-hand) and those not. Interestingly, coefficients of the effect of benefits B of larger distance in the left of the table are larger for credit constrained agents, suggesting the existence of such effects.

Table 3.17: Cox-model Estimates, Excluding Large Cities But Vienna

	$(w+, d+)$	$(w-, d+)$	$(w_0, d+)$	$(w+, d-)$	$(w-, d-)$	$(w_0, d-)$	$(w+, d_0)$	$(w-, d_0)$	(w_0, d_0)
B	-0.306***	-2.633***	-1.381***	-0.330***	-2.612***	-1.479***	-0.627***	-2.709***	-1.920***
	(0.060)	(0.076)	(0.114)	(0.070)	(0.084)	(0.133)	(0.114)	(0.123)	(0.168)
b	-0.342***	-0.616***	-0.275	-0.372***	-0.454***	-0.716***	-0.258	-0.351**	-0.537
	(0.096)	(0.082)	(0.184)	(0.112)	(0.092)	(0.241)	(0.203)	(0.158)	(0.356)
PBD [weeks]	-0.006**	0.004*	-0.008*	-0.003	0.009***	0.002	-0.003	0.009**	0.008
	(0.003)	(0.003)	(0.005)	(0.003)	(0.003)	(0.006)	(0.005)	(0.004)	(0.007)
Nuts3 FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spells	104,885	104,885	104,885	104,885	104,885	104,885	104,885	104,885	104,885
Individuals	81,637	81,637	81,637	81,637	81,637	81,637	81,637	81,637	81,637
Log L	-226692	-225306	-60907	-155699	-176464	-43622	-60643	-66608	-26272
Share Exits	0.21	0.21	0.06	0.14	0.16	0.04	0.06	0.06	0.02

Notes: Duration variable is nonemployment in months. Estimates refer to coefficients. w_0 contains changes in wage of $+/-$ 4%. Control Variables: Potential Benefit Duration, net wage used for calculation of replacement rate. Experience in the last two, five and ten years (5 is net of 2 and 10 net of 5 years), altitude of the municipality of residence, time to the next large city, age in years, real wage and occupation of the last job before unemployment, marital status and number of children. Voluntary quits and recalls are excluded, only Replacement Rates weakly below 1 and potential benefit durations above 0 are considered. Standard errors are clustered on individual level. Significance is indicated as follows: * (p<0.1), ** (p<0.05), *** (p<0.01).

Table 3.18: Cox-model Estimates, Excluding the Retail Sector

	(w+d+)	(w-d+)	(w0d+)	(w+d-)	(w-d-)	(w0d-)	(w+d0)	(w-d0)	(w0d0)
B	-0.281***	-2.496***	-1.283***	-0.267***	-2.565***	-1.428***	-0.585***	-2.404***	-1.814***
	(0.052)	(0.067)	(0.099)	(0.060)	(0.072)	(0.112)	(0.088)	(0.097)	(0.142)
b	-0.258***	-0.592***	-0.319**	-0.310***	-0.589***	-0.679***	-0.354**	-0.495***	-0.684***
	(0.078)	(0.067)	(0.150)	(0.091)	(0.073)	(0.188)	(0.148)	(0.116)	(0.264)
PBD [weeks]	-0.006***	0.004*	-0.010**	-0.003	0.008***	0.002	-0.001	0.007**	0.009
	(0.002)	(0.002)	(0.004)	(0.003)	(0.002)	(0.005)	(0.004)	(0.003)	(0.006)
Nuts3 FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spells	137,794	137,794	137,794	137,794	137,794	137,794	137,794	137,794	137,794
Individuals	106,936	106,936	106,936	106,936	106,936	106,936	106,936	106,936	106,936
Log L	-282000	-298343	-77825	-206432	-249117	-59824	-90805	-105202	-35576
Share Exits	0.19	0.20	0.05	0.14	0.17	0.04	0.06	0.07	0.02

Notes: Duration variable is nonemployment in months. Estimates refer to coefficients. w0 contains changes in wage of +/− 4%. Control Variables: Potential Benefit Duration, Netwage used for calculation of replacement rate. Experience in the last two, five and ten years (5 is net of 2 and 10 net of 5 years), altitude of the municipality of residence, time to the next large city, age in years, real wage and occupation of the last job before unemployment, marital status and number of children. Voluntary quits and recalls are excluded, only Replacement Rates weakly below 1 and potential benefit durations above 0 are considered. Standard errors are clustered on individual level. Significance is indicated as follows: * ($p<0.1$), ** ($p<0.05$), *** ($p<0.01$)

Table 3.19: Cox-Model Estimates, Sub-Hazards by Previous Tenure

		Benefits			Assistance		
		$d+$	d_0	$d-$	$d+$	d_0	$d-$
Constraint	$w+$	1.680***	1.366***	1.065***	0.001	0.066	-0.523
	w_0	0.669***	0	-0.008	0.382	0	-0.603
	$w-$	-0.250	-0.019	-0.695***	0.504	0.735	0.321
Unconstraint	$w+$	1.468***	1.150***	1.498***	0.649**	0.634**	0.737**
	w_0	0.458***	0	0.380**	0.538*	0	0.439
	$w-$	-0.675***	-0.587***	-0.704***	0.127	0.310	0.235

Notes: The table summarizes estimates from a competing risk Cox regression to each combination of wage and distance destination. The table reports coefficients on unemployment benefits (B) and unemployment assistance (b) relative to the coefficient estimated for the constant wage and same municipality of residence destination (w_0,d_0). Significance is indicated as follows: *(p<0.1), **(p<0.05), *** (p<0.01). Unconstraint : people with at least 3 years of tenure on the job before UI (getting 2 months of salary as cash-on-hand). Constraint : other.

4 Financial Incentives and Earnings of Disability Insurance Recipients: Evidence from a Notch Design

Joint with Stefan Staubli

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4.1 Introduction

Disability Insurance (DI) programs are among the largest social insurance programs. In OECD countries, total expenditures on disability benefits account for approximately 2.5 percent of GDP on average (OECD, 2010b). DI programs are designed to provide income replacement in the case of a permanent loss of earnings capacity due to poor or deteriorating health, but there have been concerns that DI discourages work. A work disincentive that exists in many DI programs is the policy that beneficiaries lose part or all of their benefits if earnings exceed a substantial gainful activity (SGA) amount. The loss of benefits at the SGA threshold – also referred to as the "cash cliff" – induces a high implicit tax on work and creates an incentive for beneficiaries to keep their earnings below the SGA threshold in order to retain benefits.

If "parking" just below the SGA threshold is widespread, then policies that relax the threshold could increase earnings among DI beneficiaries, potentially improving their economic well-being and their autonomy while reducing dependency on benefits.[1] Yet, these policies could create unintended costs if they induce more individuals to apply for and ultimately receive disability benefits. If, instead, few beneficiaries reduce earnings because of the SGA threshold, then efforts to lower the implicit tax on work are likely to have small impacts on earnings and benefits. Despite numerous anecdotes of beneficiaries intentionally keeping their earnings just below the SGA threshold, there is little empirical evidence on the impact of the SGA threshold, and financial incentives in general, on earnings of beneficiaries.[2]

This paper helps to fill this gap by investigating whether earnings thresholds induce DI recipients to adjust their earnings as well as providing an estimate of an earnings elasticity to financial incentives. Our estimation strategy exploits quasi-experimental variation in the implicit tax on work in the DI program in Austria. Specifically, DI beneficiaries in Austria can earn up to an SGA threshold of €439 per month (around USD 500) without losing benefits. If monthly earnings exceed the SGA threshold by €1, then DI benefits are reduced by up to 50 percent in that month. These rules generate a discontinuous increase in the (implicit) tax liability – a notch – at the SGA threshold and create a strong incentive for many DI beneficiaries to "bunch" on the low-earnings side of the SGA threshold.[3] The amount of bunching can be used to estimate the elasticity of earnings with respect to the net-of-tax rate, as shown by Saez (2010).

Observed bunching might be attenuated if some individuals do not respond due to optimization frictions such as adjustment costs and inattention. The long-run or structural response absent frictions is likely to be larger. One advantage of our notch design as opposed to a kink design (see, e.g., Chetty et al., 2011; Saez, 2010) is the ability to estimate a long-run earnings response. The reason is that a notch creates a region of

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[1]Many countries are considering or have recently implemented policy reforms designed to increase work incentives for DI recipients. For example, the U.S. is currently testing a benefit offset policy that reduces benefits by $1 for every $2 of earnings above the SGA threshold, rather than fully suspend benefits. Switzerland tested a conditional cash program that offered DI recipients a cash payment if they take up or expand employment and reduce disability benefits (see Bütler et al., 2014, for an evaluation of the program). Other recent examples include the United Kingdom and Norway (see Kostol and Mogstad, 2014).

[2]For example, the article "Disability Insurance: Not Working" in the magazine *the Economist* (issue from January 24, 2015) provides anecdotal evidence for such behavior in the U.S. Social Security Disability Insurance. Empirical evidence is provided byCampolieti and Riddell (2012); Kostol and Mogstad (2014); Schimmel et al. (2011); Weathers and Hemmeter (2011) (discussed in detail below).

[3]Recent studies relying on notches in the budget set examine such diverse topics as earnings adjustments to income and payroll taxes (Kleven and Waseem 2013; Tazhitdinova 2015), automaker responses to fuel economy regulations (Ito and Sallee 2014; Sallee and Slemrod 2012), the impact of transfer taxes on the real estate market (Best and Kleven, 2014; Kopczuk and Munroe, 2014), the effect of tax credits on retirement savings and income (Ramnath, 2013), the labor supply effects of social security (Manoli and Weber, 2011), and firm responses to stricter tax enforcement (Almunia and Lopez Rodriguez, 2014). Our paper contributes to this literature by studying behavioral responses at a notch in the disability benefit schedule.

strictly dominated choices on the high-earnings side of the SGA threshold. In this range, beneficiaries can increase both total net income and leisure by moving below the SGA threshold. Since without frictions no individual should locate in the dominated region, we can use the observed density mass in this region to estimate the magnitude of attenuation bias from frictions.

The SGA threshold in Austria is an appealing context to study earnings adjustment to financial incentives for at least two reasons. First, the increase in tax liability at the SGA threshold is large in magnitude and very salient. The average DI beneficiary loses around 8.5 percent of his or her total net income if earnings exceed the SGA threshold by €1. Detecting behavioral responses would be more difficult in other contexts because earnings rules are typically more complex and therefore less salient.[4] The large variation in tax liability facilitates the identification of behavioral responses to financial incentives even if the responses are small. Second, bunching below the SGA threshold is often difficult to detect in administrative data, because earnings are measured at the annual level while the SGA threshold is specified at the monthly level. Hence, recipients who bunch at the SGA threshold only for some months of the year would not appear to bunch in annual data. We rely on very detailed administrative data from social security and tax registers, allowing us to precisely measure earnings and DI benefits at the monthly level. Moreover, since sample sizes are very large, we can graphically demonstrate bunching, providing transparent evidence of a behavioral response.

The insights from our empirical analysis can be summarized by five broad conclusions. First, there is large and sharp excess bunching in earnings of DI beneficiaries just below the SGA threshold. We estimate that DI beneficiaries who earn just below the SGA threshold would increase monthly earnings by up to €400 if the notch at the SGA threshold did not exist. This represents a 91 percent increase relative to the SGA earnings level. Second, bunching is very persistent over time; almost 60 percent of those who bunch after entering the program do so five years later. Third, observed bunching is attenuated by frictions, as many beneficiaries are located in the dominated range. Yet, over time beneficiaries leave the dominated range quickly and the majority moves to the SGA threshold. Fourth, even though the estimated earnings response is large, the implied earnings elasticity accounting for frictions is small with 0.206. This elasticity estimate masks significant heterogeneity in the responsiveness to financial incentives across subgroups. Specifically, we find that

[4] For example, beneficiaries in the public DI program in the U.S. can earn above SGA for nine months (not necessarily consecutive) over any five-year period. After exhausting the nine-months period, beneficiaries enter the extended period of eligibility (EPE). If earnings are above the SGA threshold during the EPE, benefits are paid for three additional months, but are suspended in full thereafter during each month that beneficiaries earn above SGA. If earnings are above the SGA three years after entering the EPE, benefits are terminated. Chetty et al. (2009) provide evidence that individuals are not as responsive to less salient policies compared to more salient policies.

women and younger age groups are more responsive to financial incentives compared to men and older age groups. Fifth, simulations show that replacing the notch with a €1 benefit offset for every €2 of earnings would increase work and reduce benefit payments among current beneficiaries. However, overall government expenditures would likely increase to the extent that such a policy would induce more individuals to seek DI benefits.

To assess the generalizability of our results, we complement our empirical analysis by comparing the estimates of the work capacity of DI beneficiaries in Austria to other countries. We follow the approach suggested by Bound (1989) who uses the labor force participation rate of rejected DI applicants as an estimate of the labor force participation rate of DI beneficiaries had they not received benefits. Applying this approach to Austria, we obtain estimates that are similar to the OLS estimates reported in Maestas et al. (2013) for the U.S and Kostol and Mogstad (2014) for Norway. Nevertheless, caution applies when extending our findings to other countries. Our estimation approach exploits variation in earnings of beneficiaries located around the SGA threshold in Austria. In countries with different SGA thresholds characteristics of beneficiaries around the SGA thresholds may differ, which could result in different elasticity estimates.

Our paper is primarily related to the literature that studies the effects of policy reforms to increase work incentives for DI beneficiaries. Hoynes and Moffitt (1999) simulate the financial impacts of a number of potential reforms and conclude that the effects on work effort are often not as strong as expected. Consistent with this view, Schimmel et al. (2011) find that a small increase in the monthly SGA threshold from USD 500 to USD 700 in the U.S. had only a modest impact on earnings of DI beneficiaries. However, more recent evidence suggests that some policies appear to be quite effective in increasing employment. Campolieti and Riddell (2012) find that the introduction of an earnings exemption of CAD 3,800 per year in Canada led to a significant increase in disability beneficiaries' propensity to work, but did not have an effect on program inflow or outflow. Weathers and Hemmeter (2011) and Kostol and Mogstad (2014) find that replacing the cash cliff with a gradual reduction in benefits leads to a significant increase in work effort of DI beneficiaries. Our contribution is to provide the first empirical evidence of bunching at the SGA threshold – a work disincentive that is present in many DI programs – and to document the dynamics of earnings adjustment over time.

Our paper is also related to the literature on the work potential of disability beneficiaries (e.g., Bound, 1989; Chen and van der Klaauw, 2008; French and Song, 2014; Maestas et al., 2013; von Wachter et al., 2011). Since these studies use rejected applicants as a control group to estimate the extent to which DI benefits distort work effort, they therefore provide a good estimate for the employment potential of beneficiaries at the time of applying. Yet, there is much less evidence on the employment potential of beneficiaries who have been on the program for some time. Prior literature also focuses primarily on the impact of the DI program on labor force participation and has not examined intensive responses, which is the focus of this paper.

This paper proceeds as follows. Section 4.2 describes Austria's DI program. Section 4.3 outlines the bunching methodology, summarizes the data, and presents descriptive statistics. Section 4.4 shows descriptive evidence for bunching at the SGA threshold, presents our estimates for the earnings elasticities, and simulates the fiscal effects of hypothetical reforms. Section 4.5 concludes.

4.2 Institutional Background

4.2.1 The Austrian DI Program

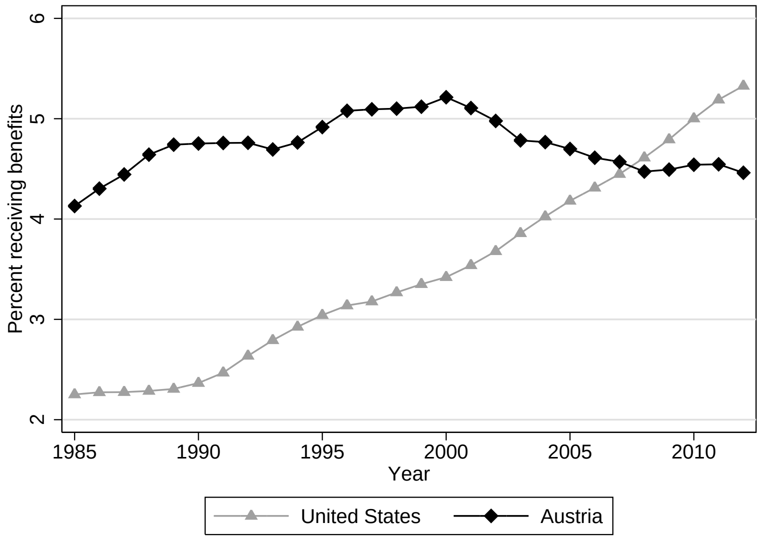
The Austrian DI program is part of the larger social security system that is financed by a payroll tax on earned income. The program provides partial earnings replacement to workers below the full retirement age who are unable to engage in substantial gainful activity due to a medically determinable health impairment that has lasted for at least six months. As Figure 4.1 shows, the percentage of the working age population receiving DI benefits in Austria has been relatively constant at 4.3 percent to 5.2 percent from 1985 to 2012, while the rate of DI receipt in the U.S. increased from 2.2 percent to 5.3 percent over the same time period.[5]

To apply for DI benefits, an individual must submit an application to the DI office in their state of residence (there are nine states in Austria). Employees at the DI office first check the non-medical eligibility criteria for DI benefits. Only individuals who have contributed to the program for at least 5 years in the past 10 years and are not yet eligible for retirement benefits can apply for DI benefits. DI eligibility in Austria is not conditioned on earnings, so individuals can continue to work while they apply for and receive benefits. If an applicant meets the nonmedical criteria, a team of disability examiners and physicians assesses the applicant's overall ability to work and the medical severity of the applicant's disability. A disability award is made if the medical examination finds that a medically determinable impairment causes more than 50 percent of a reduction in ability to work relative to that of a healthy person with comparable education.[6] If the

[5]Other countries have experienced similar or even more striking increases in disability recipiency rates as the U.S., from 2 percent to around 6 percent in Australia and Ireland, from 3 to 6 percent in the U.K., and from 6 to 10 percent in Norway.

[6]Medical criteria for disability classification are relaxed starting at age 57. See Staubli (2011) for the impact of this relaxation on labor force participation of older workers.

Figure 4.1: Disability Insurance Recipiency per Adult Ages 25-64



Source for Austria: STATISTIK AUSTRIA population data; statistical supplement published by "Hauptverband der Österreichischen Sozialversicherungstraeger". Source for the United States: Social Security Bulletin: Annual Statistical Supplement; Bureau of the Census, Census Population Estimates, available at `http://www.census.gov/programs-surveys/popest.html`.

health impairment is expected to be temporary, DI benefits are granted for a limited time period of typically two years. DI benefits are awarded for an indefinite time period in case of permanent health impairments. Applicants who disagree with the decision of the DI office can appeal within three months.

Once DI benefits are awarded, there are three main pathways out of the program. First, DI claimants may no longer meet the medical or non-medical eligibility criteria for disability benefits. For example, the health status may improve such that the DI recipient is no longer disabled. In 2012, medical improvements and return to work accounted for 88.4 percent of program exits. Second, DI claimants may reach the full retirement age, at which point they can ask to be transferred to the old-age pension program. However, few beneficiaries do so because in most cases the corresponding old-age pension would be lower than the disability pension. In 2012, 8.7 percent of those who left the DI program were shifted to the old-age pension program. Third, the DI recipient may die. Death accounted for 2.9 percent of program exits in 2012. In 2012, the DI exit rate stood at 1.6 percent which is lower than in many other countries. For example, the exit rate in the U.S. Social Security Disability Insurance is four times higher (Moore, 2015).

DI benefits are fairly generous and replace about 60 percent of pre-disability earnings up to a maximum of approximately €2,800 per month (around USD 3,150). Benefits are subject to income tax and mandatory health insurance contributions. The level of benefits depends on an assessment basis and a pension coefficient. The assessment basis corresponds to the average earnings over the best 20 years after applying a cap to earnings in each year. The pension coefficient is the percentage of the assessment basis that is received in the pension. The pension coefficient increases with the number of contribution years up to a maximum of 80 percent (roughly 45 contribution years). Applicants under age 60 qualify for a special increment if their pension coefficient is below 60 percent.

4.2.2 The Substantial Gainful Activity Threshold

Like in the United States and other countries, DI beneficiaries in Austria can earn up to a Substantial Gainful Activity (SGA) threshold without losing any benefits. All earnings are subject to regular income tax. In 2012, the monthly SGA threshold in Austria was €439 (around USD 500), which is about half of the SGA threshold for non-blind DI recipients in the U.S. (USD 1,010 in 2012). However, DI recipients lose a fraction of their benefits in each month in which earnings exceed the SGA threshold. The loss in benefits if a beneficiary earns above the SGA threshold in a given month depends on the sum of benefits and earnings in that month and is calculated as follows:

$$\triangle s = \begin{cases} 0 & \text{if } s+z \leq K_1 \\ 0.3(s+z-K_1) & \text{if } K_1 < s+z \leq K_2 \\ 0.3(K_2-K_1)+0.4(s+z-K_2) & \text{if } K_2 < s+z \leq K_3 \\ 0.3(K_2-K_1)+0.4(K_3-K_2)+0.5(s+z-K_3) & \text{if } s+z > K_3, \end{cases} \tag{4.1}$$

where s denotes monthly before-tax DI benefits and z are monthly before-tax earnings. The values K_1, K_2, and K_3 are adjusted each year to account for inflation; in 2012 the corresponding values were €1,258, €1,887, and €2,515. Equation 4.1 illustrates that the reduction in benefits Δs is increasing in s and z. However, the maximum reduction is capped at 50 percent of full benefits; thus DI recipients are always allowed to keep $0.5s$ independent of how much they earn. The SGA threshold coincides with the earnings threshold above which workers are automatically insured by the public pension system. DI recipients with earnings above the SGA threshold are therefore required to pay social security contributions on *all* earnings. The social security tax is 18 percent for workers and 21 percent for employers.

Together, these rules change the implicit tax on work at the SGA threshold in two ways. First, there is a discrete jump in the overall tax liability—a notch—because beneficiaries lose a fraction of their benefits and their earnings on the first Euro of earnings above the SGA threshold. The average beneficiary loses about €100, or 8.5 percent of total after-tax income, of which 70 percent are due to the benefit loss and 30 percent are due to social security contributions. Second, there is a discrete change in the implicit marginal tax—a kink—because for each Euro of earnings above the SGA threshold beneficiaries lose between 30-50 cents in benefits, as illustrated in equation (4.1), and they have to pay 18 cents in payroll taxes.

Both the notch and the kink create a strong incentive for DI recipients to bunch just below the SGA threshold in order to avoid the high implicit tax on work and retain full DI benefits. In the next section, we will describe our methodology how we combine the amount of bunching with the change in the implicit tax at the SGA threshold to estimate an elasticity of earnings with respect to the implicit net-of-tax rate.

4.3 Methodology and Data

4.3.1 Theoretical Framework

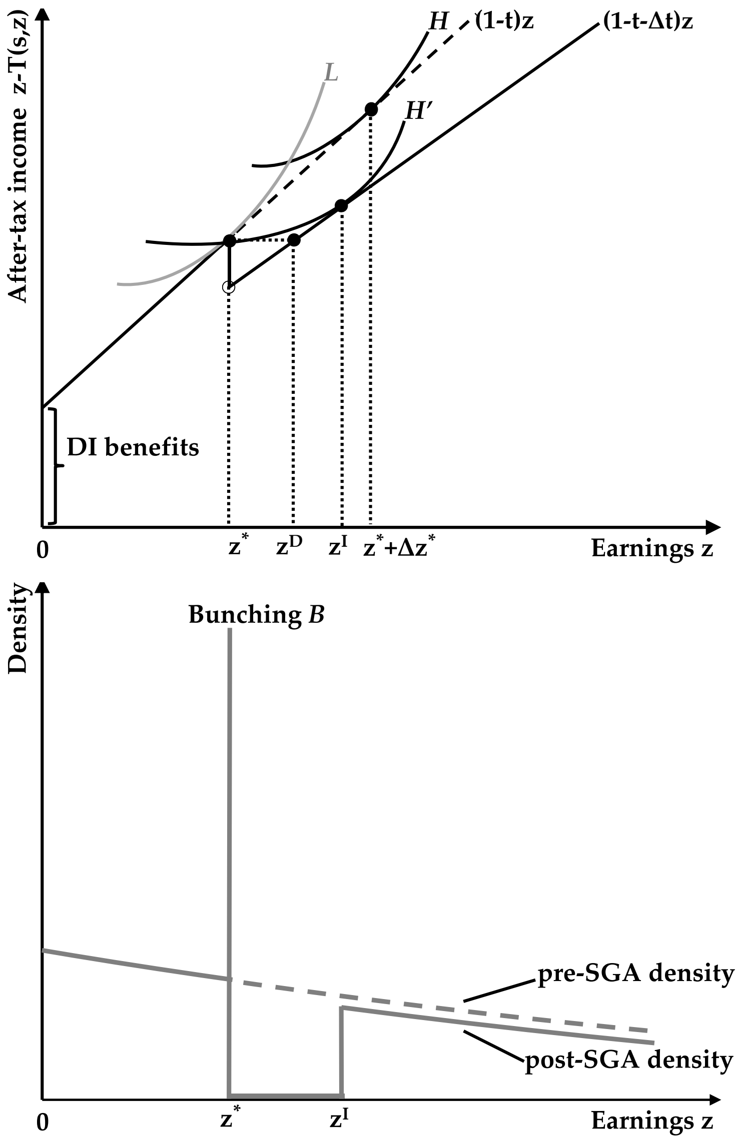
A series of recent studies estimate the income elasticity of taxpayers by exploiting kinks and notches in the income tax schedule (Chetty et al. (2011); Kleven and Waseem (2013); Saez (2010)). We begin with a model that follows Kleven and Waseem (2013) with the difference that we focus on a notch in disability insurance and not in the income tax schedule. Specifically, suppose that individual preferences are described by a quasi-linear and iso-elastic utility function of the form

$$u(z) = z - T(s, z) - \frac{n}{1 + 1/e}\left(\frac{z}{n}\right)^{1+1/e} \tag{4.2}$$

where $T(s, z)$ is the tax liability, n is an ability parameter, and e is the elasticity of earnings with respect to the marginal net-of-tax rate $1 - t$. This specification rules out income effects and below we also present an alternative approach that does not rely on a specific form of the utility function. In the counterfactual case of a linear tax system $T(s, z) = -(1 - t)s + tz$, the distribution of ability, which is assumed to be smooth, translates into a smooth distribution of earnings.

Now suppose that a notch and a kink are introduced at the earnings threshold z^*, representing the SGA threshold. The tax schedule with the notch and the kink can be written as $T(s, z) = -(1 - t)s + tz + [\Delta T + \Delta tz]\mathbf{1}(z > z^*)$ where ΔT is the size of the notch, Δt is the size of the kink, and $\mathbf{1}(z > z^*)$ is an indicator for earning above the SGA threshold. The upper panel of Figure 4.2 shows that the notch shifts the budget constraint downward at z^* while the kink rotates the budget constraint. As a consequence, all DI beneficiaries who earned in the interval $(z^*, z^* + \Delta z^*)$ before the SGA threshold is introduced would instead move to z^*, implying that the earnings distribution with the SGA threshold exhibits bunching at z^*, as shown in the lower panel of Figure 4.2. Moreover, the earnings distribution with the SGA threshold features a hole because the notch creates a region of strictly dominated choices between z^* and z^I. The earnings distribution above z^* also shifts to the left because the kink induces all DI recipients to earn less.

Figure 4.2: Budget Sets and Density Distributions



Notes: The figure shows after-tax monthly income as a function of monthly gross earnings; z^* denotes the SGA threshold; z^D denotes the earnings level at which the after-tax income is equal to the after-tax income at the SGA threshold.

The width of the earnings response Δz^* is proportional to the size of the kink-notch and the elasticity e. Since the tax parameters are known, we only need to estimate Δz^* to uncover an estimate for the elasticity e. As shown by Saez (2010), the amount of bunching B observed at kinks can be used to identify Δz^*. The estimate of the earnings response from a kink likely captures a short-run effect if some individuals do not respond because they face optimization frictions such as adjustment costs and inattention; the long-run, structural response absent frictions might be substantially larger.[7] A key advantage of a notch is that it allows for the estimation of a long-run earnings response. The reason is that no individual should choose an earnings level in the dominated range and any mass in the dominated range should therefore be the result of adjustment frictions. Following Kleven and Waseem (2013), we determine the long-run earnings response Δz^* as the point of convergence between the observed and the counterfactual earnings distribution in the absence of a kink-notch (the next section describes how we estimate the counterfactual earnings distribution).

The approach to pin down Δz^* implicitly assumes that the observed mass in the bunching segment $(z^*, z^* + \Delta z^*)$ is driven by frictions and that the excess mass at the SGA threshold is coming from the area between the observed and the counterfactual distribution. With heterogeneity in elasticities, it is possible that some of the mass in the bunching segment is explained by low elasticities and not frictions, in which case the convergence point overestimates the true Δz^*. Moreover, in a dynamic setting the loss in current income from being in the bunching segment may be compensated by higher future earnings through career effects. In the empirical application, we will shed light on the size of the bias created by low elasticities and career effects by examining the dynamics of earnings adjustment. More specifically, we expect that over time individuals subject to frictions or career effects will move away from the bunching segment while those with low elasticities will stay.

Finally, to estimate the earnings elasticity e, we exploit the fact that the marginal buncher is indifferent between the SGA threshold z^* and the interior point z^I, as shown in Figure 4.2. Thus, we can set $u(z^I) = u(z^*)$ using equation (4.2) and rearrange terms to obtain the following expression which defines the elasticity e as an implicit function of the tax parameters, the SGA threshold z^*, and the earnings response Δz^* (see Appendix D for derivation):

$$\frac{1}{1+\Delta z^*/z^*}\left[1+\frac{\Delta T/z^*-\Delta t}{1-t}\right]-\frac{1}{1+1/e}\left(\frac{1}{1+\Delta z^*/z^*}\right)^{1+1/e} \\ -\frac{1}{1+e}\left(1-\frac{\Delta t}{1-t}\right)^{1+e}=0. \qquad (4.3)$$

Equation (4.3) cannot be solved explicitly for e, but it can be solved numerically after we have estimated Δz^*since the tax parameters and the SGA threshold are known.

One drawback of the above approach is that it relies on a specific functional form for

[7]Gelber et al. (2013) show that it is possible to estimate a structural earnings response from kinks by exploiting policy-changes in the magnitude or the location of the kinks. More specifically, they estimate adjustment costs using the speed by which earnings adjust to policy changes.

utility. To relax this assumption, we also implement a reduced-form approach following Tazhitdinova (2015) that does not depend on the structure of the underlying utility. The key idea is to split the amount of bunching B into bunching generated by the notch and bunching generated by the kink, assuming that the same elasticity is driving both responses. To be able to estimate the fraction of bunching from the notch and the kink, we first need to guess the elasticity e_0. After estimating the counterfactual earnings distribution, we can then convert each bunching response into a corresponding earnings response, denoted by Δz^*_{kink} for the kink and Δz^*_{notch} for the notch. In the case of the kink, we can back out the earnings elasticity by relating the estimated earnings response Δz^*_{kink} to the change in the marginal tax rate Δt using the definition of the earnings elasticity with respect to the net-of-tax rate

$$e = \frac{\Delta z^*_{kink}/z^*}{\Delta t/(1-t)} \tag{4.4}$$

Applying the same logic is less straightforward for the notch, because the notch induces a jump in the average tax rate rather than the marginal tax rate. However, Kleven and Waseem (2013) show that it is possible to approximate the size of the notch ΔT by an equivalent increase in the implicit marginal tax rate from t to

$$t^* = \frac{T(s, z^* + \Delta z^*_{notch}) - T(s, z^*)}{\Delta z^*}. \tag{4.5}$$

With this approximation we can back out the elasticity using equation (4.4). The resulting elasticity estimate $\hat{e}$ will typically not match the initial guess e_0. We therefore update the initial guess to $\hat{e}$ and repeat the estimation procedure to get a new elasticity estimate. We continue this iterative process until it converges to a fixed point $\hat{e} = e_0$.

4.3.2 Empirical Implementation

Our framework relies on a credible identification of the counterfactual earnings density—the distribution of earnings under a linear tax system without any notch or kink. Following Kleven and Waseem (2013), we begin by grouping DI recipients into earnings bins of €10 based on their monthly earnings. We proceed by estimating a flexible polynomial to the observed earnings distribution, excluding observations in a range $[z^L, z^U]$ below and above z^*:

$$C_j = \sum_{i=0}^{p} \beta_i (z_j)^i + \sum_{k=z^L}^{z^U} \gamma_k \mathbf{1}(z_j = k) + \varepsilon_j, \tag{4.6}$$

where C_j is the number of individuals in bin j, z_j is the earnings level in bin j, and

p is the order of the polynomial. The excluded range $[z^L, z^U]$ corresponds to the area that is affected by the SGA threshold either because of excess bunching or missing mass. Because we include indicator variables for each bin in the excluded range, the polynomial is estimated without considering data from the excluded range. The counterfactual distribution is given by the predicted values from equation (4.6) omiting the dummies in the excluded range: $\hat{C}_j = \sum_{i=0}^{p} \hat{\beta}_j (z_j)^i$. Excess mass is the difference between the observed and counterfactual earnings distribution in the range $[z^L, z^*]$: $\hat{B} = \sum_{k=z^L}^{z^*} (C_k - \hat{C}_k)$. Missing mass is the difference between the observed and counterfactual earnings distribution in the range $(z^*, z^U]$: $\hat{M} = \sum_{k>z^*}^{z^U} (\hat{C}_k - C_k)$.

Since bunching below the SGA threshold is very sharp, we can determine the lower bound of the excluded range z^L by visual inspection. A similar approach is not feasible for the upper bound z^U because missing mass is fuzzier and cannot be easily determined visually. Instead, we exploit the fact that the missing mass on the right of the SGA threshold must be equal to the bunching mass on the left of the SGA threshold. More specifically, we start by setting z^U equal to the first bin on the right of z^* and estimate the counterfactual earnings density using equation (4.6). The resulting bunching mass in this case will exceed missing mass. We then increase the upper bound in small increments and re-estimate equation (4.6) until the estimated missing mass is equal to the estimated bunching mass. Importantly, the resulting upper bound z^U directly represents our estimate for the long-run earnings response Δz^*. Plugging this estimate into equation (4.3) allows us to uncover the structural elasticity.

Estimating the counterfactual density as outlined above ignores the potential left shift in the observed distribution above the SGA threshold due to the kink, as illustrated in Figure 4.2. This assumption makes sense in Kleven and Waseem (2013) because in their setting the change in the marginal tax above the notch is small. It may not hold in our setting because the change in the marginal tax above the SGA threshold is large. Not accounting for this kink may bias the counterfactual distribution downward in which case $\hat{B}$ would overestimate the true excess mass. To examine this concern, we estimate a version of the counterfactual density that accounts for the kink by shifting the density on the right of z^U upward until the area under the counterfactual and the empirical distribution is identical. It turns out that the elasticity is similar independent of the estimation approach for the counterfactual density.

Since notches introduce a discrete jump in tax liability, they may induce some individuals to stop working altogether. Such extensive responses would lower the counterfactual density, introducing an upward-bias in our estimate of the excess mass, because fewer individuals are located to the right of the SGA threshold. The size of the bias is likely to be small because our estimation approach relies on excess mass and missing mass in a narrow range around the SGA threshold, while extensive responses around the SGA threshold will be small. The reason is that recipients, who would locate just above z^* in the absence of the earnings threshold, are likely better off by moving to z^* in the presence of the earnings threshold because z^* is almost as good as the pre-threshold state.

We calculate standard errors for all estimated parameters using a bootstrap procedure, as in Chetty et al. (2011). Specifically, we generate a large number of earnings distributions by random resampling with replacement from the population and calculate new estimates for Δz^* and e by applying the above techniques. We define the standard error of Δz^* and e as the standard deviation of the distribution of estimates.

4.3.3 Data and Sample Selection

We combine register data from two different sources. First, the Austrian Social Security Database (ASSD) contains very detailed longitudinal information for all private sector workers in Austria between 1972 and 2012. At the individual level the data include gender, nationality, month and year of birth, blue-collar or white-collar status, and labor market history. Labor market histories are summarized in spells. Specifically, the start and end dates of all employment, unemployment, disability, sick leave, and retirement spells are recorded. The data contain several firm-specific variables: geographical location, industry affiliation, and firm identifiers that allow us to link both individuals and firms. Second, we use income tax reports that firms and the social security administration are required to submit to the tax office at the end of each year. These reports contain detailed information on benefits from the various social insurance programs, earnings, social security contributions, and income tax withholdings for the tax office. We have access to the tax records for the years 1994 to 2012 which can be linked with the ASSD via an identifier variable.

To investigate the effect of the SGA threshold on earnings, we consider all DI spells that were initiated between 2001 and 2012 by individuals younger than age 57 at the time of entry into the program. We exclude spells that started prior to 2001 because earnings restrictions were not uniformly regulated for these spells. We focus on DI recipients who are younger than age 57 because individuals who start claiming benefits after age 57 face stricter earnings restrictions. These individuals lose all benefits if earnings exceed the SGA threshold and they are not allowed to work in the same occupation as before the onset of the disability. We observe individuals at a monthly frequency up to eight years before they enter the DI program, while they are on the DI program, and up to four years after they exit the DI program (in case program eligibility ceased due to medical recovery). Having data at the monthly level is crucial given that the SGA limit applies monthly.[8]

Table 4.1 provides summary statistics for our analysis samples. Column 1 shows summary statistics for all DI recipients in our sample, columns 2 shows summary statistics for DI recipients who work at least once during the observation period, and column 3 shows summary statistics for the subset of DI recipients who are working just below the SGA threshold. DI recipients are on average 48.2 years old at program entry and 59 percent suffer from a musculoskeletal disease or a mental disorder which are typically difficult to verify. [9] A comparison of columns 1 and 2 shows that only about 15 percent of DI beneficiaries are working while receiving benefits. Compared to all DI recipients, working DI recipients are younger, had a lower wage in their last job, have lower DI benefits, have more labor market experience, and suffer less from difficult-to-verify disorders. On average, they earn about 50 percent less than what they earned before entering the DI program. This drop is explained to a large extent by the fact that many DI beneficiaries are earning just below the SGA threshold; column 3 illustrates that over 25 percent of working DI beneficiaries are located just below the SGA threshold.

[8]It would be harder to detect bunching with annual earnings data because beneficiaries who earn just below the SGA for several months (but not the whole year) would not appear to bunch in annual data. Figure 4.8 in Appendix B shows the distribution of annual earnings around the SGA limit. While there is clear evidence for bunching at the SGA threshold, the amount of bunching is an oder of magnitude lower than in monthly data (see Fig 4.3).

[9]The high fraction beneficiaries with difficult-to-verify disorders is typical for DI programs in many countries. For example, the fraction of recipients suffering from a mental illness or a musculoskeletal disease is 57.4 percent in the U.S. Social Security DI program (Maestas et al. (2013)) and 61.4 percent in Norway (Kostol and Mogstad (2014)).

Table 4.1: Summary Statistics

	All DI recipients	Working DI recipients	
		All	At notch
	(1)	(2)	(3)
Female (percent)	45	45	48
Age at DI entry (years)	48.2 (8.0)	46.6 (9.0)	45.3 (9.1)
Blue-collar (percent)	67	68	62
UI duration last 15 years	1.12 (1.31)	0.93 (1.19)	1.14 (1.21)
Experience last 15 years	9.67 (4.71)	11.1 (4.00)	10.3 (4.00)
Sick leave last 15 years	0.71 (0.79)	0.60 (0.71)	0.69 (0.75)
Monthly DI benefits (in €)			
Full DI benefits	974 (498)	920 (472)	1,040 (490)
Partial DI benefits	964 (507)	688 (584)	1,040 (490)
Monthly gross earnings (in €)			
Last job before DI	3,009 (5,674)	2,411 (3,766)	1,992 (3,916)
While claiming DI	54 (533)	1,179 (2,227)	375 (43)
Health impairment			
Mental disorders (percent)	40	38	44
Musculoskeletal system (percent)	19	16	16
Cardiovascular system (percent)	10	10	8
Other (percent)	31	36	31
No. of individuals	183,168	27,054	7,084
No. of observations	7,562,737	334,461	84,787

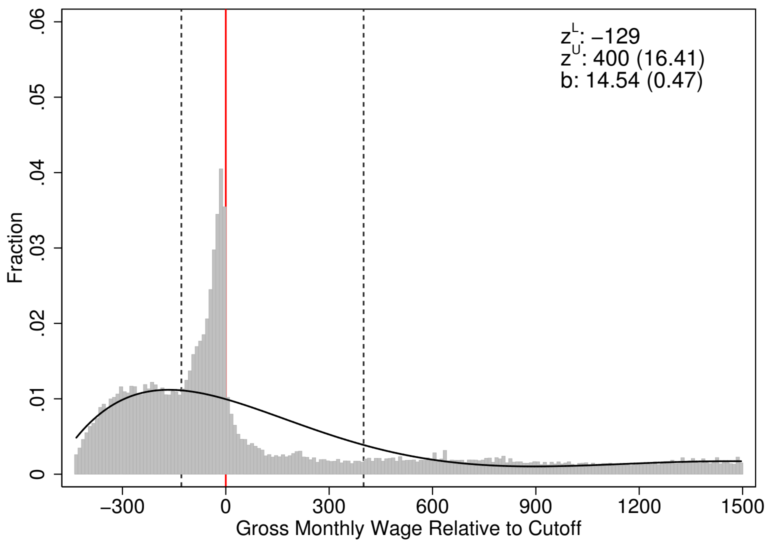
Notes: UI duration last 15 years, experience last 15 years and sick leave last 15 years are measured prior to DI entry. Sample standard deviations for continuous variables in parentheses. Monthly DI benefits and monthly gross earnings are measured during DI. Health impairment is only observed for DI spells that start in 2004 or after.

4.4 Empirical Analysis

4.4.1 Descriptive Evidence of Behavioral Responses

Evidence for Bunching in Pooled Data. We start our analysis by examining graphically whether there is evidence for bunching at the SGA threshold. To do so, we pool all available years of data and calculate the difference between earnings and the SGA threshold in a given year, given that the SGA threshold increases from year to year by about €10 to account for inflation and wage growth. We then group individuals into €10 bins and quantify excess mass and missing mass by estimating a sixth-degree polynomial to the observed earnings distribution using equation (4.6).[10] Figure 4.3 shows the normalized earnings distributions around the SGA threshold as well as our estimate of the counterfactual earnings density (black line). The vertical solid line denotes the SGA threshold and the vertical short-dashed lines denote the excluded range $[z^L, z^U]$.

Figure 4.3: Earnings Distribution Around the SGA Threshold



Notes: The figure shows the earnings distribution of monthly gross earnings around the SGA threshold (marked by the vertical solid line) for DI beneficiaries between 2001 and 2012. The excluded range $[z^L, z^U]$ is marked by vertical dotted lines. The histogram bins are monthly gross earnings relative to the SGA threshold in the relevant year. The bin width is €10. The solid line beneath the empirical distribution is a sixth-degree polynomial fitted to the empirical distribution using equation (4.6). Bunching b is excess mass in the excluded range below the notch relative to the average counterfactual density in the interval $[z^L, z^*]$ and z^U has been estimated such that missing mass equals bunching mass. Bootstrapped standard errors are shown in parentheses.

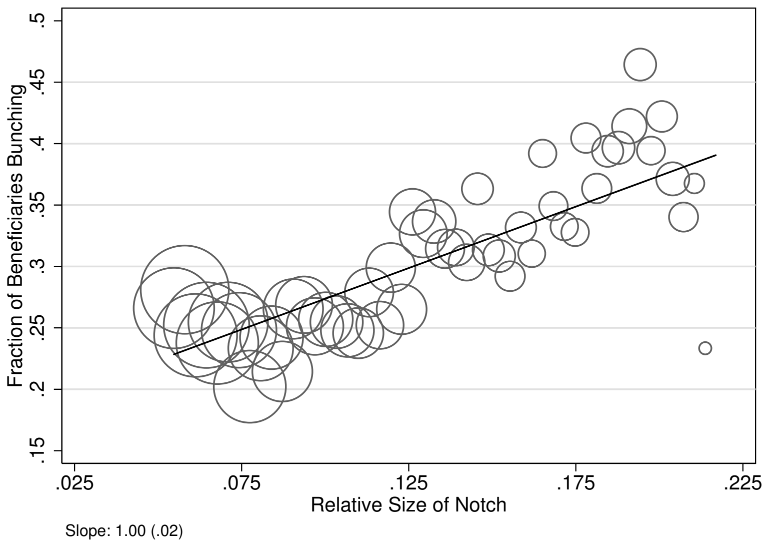
Several things can be observed from the figure. First, there is large and sharp bunching at the SGA threshold. We measure excess bunching relative to the average counterfactual density between z^L and z^*: $\hat{b} = \frac{\hat{B}}{\sum_{k=z^L}^{z^*} \hat{C}_k / ((z^* - z^L)/10 + 1)}$. Excess bunching is 14.54 times the average height of the counterfactual density and precisely estimated. Second, the earnings

[10] Figure 4.9 in Appendix B plots the counterfactual earnings distribution for lower and higher polynomial degrees, showing that the results are not very sensitive to the choice of the degree of polynomial.

distribution exhibits significant missing mass given that the density falls discretely above the SGA threshold. However, there are no visible holes as the distribution of earnings is relatively flat above the SGA threshold, suggesting that frictions are an important factor that prevent DI beneficiaries from adjusting their earnings. Third, the SGA threshold significantly reduces earnings of DI beneficiaries. The point of convergence z^U where missing mass equals bunching mass is €400, suggesting that without the SGA threshold beneficiaries who bunch would earn up to €400 more.

As equation (4.1) illustrates, the change in tax liability that beneficiaries experience at the SGA threshold varies depending on the level of DI benefits. For example, the drop in net-of-tax income due to notch ranges from €66 to €357 and the marginal tax rate increases between 18 and 50 percent. Theory predicts that excess bunching should be more pronounced the larger the change in tax liability. To test this prediction, we group beneficiaries into different bins as a function of the size of the notch relative to total pre-tax income and calculate the fraction of beneficiaries that are bunching in each bin.[11] Figure 4.4 shows a strong positive relationship between the relative size of the notch and the fraction of beneficiaries that are bunching. A 1 percent increase in the size of the notch leads to a 1 percent increase in the fraction of beneficiaries that are bunching.

Figure 4.4: Relative Size of the Notch and Fraction of Beneficiaries at the SGA Threshold



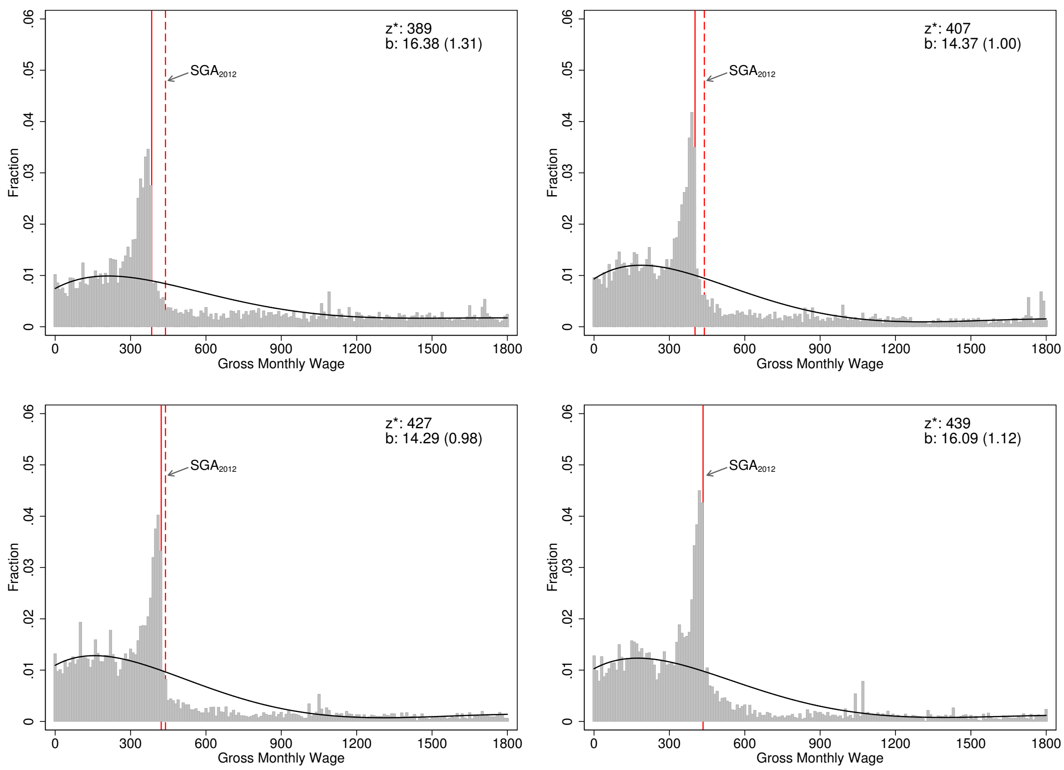
Notes: The figure shows the relationship between the size of the notch relative to the total pre-tax income (=DI benefits and earnings) and the fraction of beneficiaries that are located just below the SGA threshold, i.e. in the earnings interval $[z_L, z^*]$. Beneficiaries are grouped into bins according to the size of the notch. The size of the circles indicates the number of beneficiaries within each bin.

The identification assumption underlying our estimates for excess bunching and missing mass is that the earnings distribution would be smooth if there was no jump in the

[11]As equation (4.1) illustrates, both the size of the notch and the kink depend on total income (=DI benefits+earnings), Thus, DI beneficiaries who face a larger notch will also face a larger kink.

tax liability at the location of the SGA threshold. We can shed light on this identification assumption by exploiting the movement of the SGA threshold across years. Figure 4.5 displays the distribution of earnings around the SGA threshold for the years 2006, 2008, 2010, and 2012. We restrict the sample to DI recipients who entered the program in the five year window before the observation year, so that the number of observations is roughly constant across different years. The vertical line denotes the corresponding SGA threshold in a given year, while the vertical dashed line denotes the SGA threshold in 2012. Clearly, the excess mass follows the movement of the SGA threshold very closely.

Figure 4.5: Earnings Distribution around the SGA Threshold in 2006, 2008, 2010, and 2012



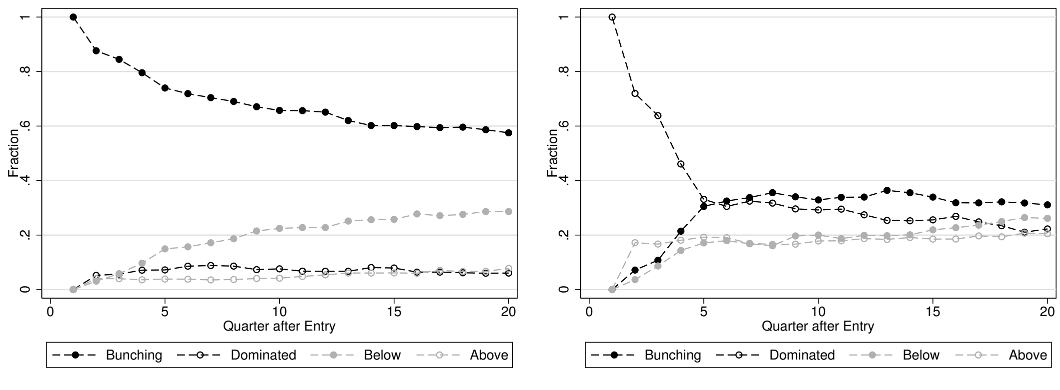
Notes: The figures show the earnings distribution of monthly gross earnings around the SGA threshold (marked by the vertical solid line) for DI beneficiaries in the years 2006, 2008, 2010, and 2012. The sample in each figure consists of DI beneficiaries who entered the program in the five year window before the observation year. The SGA threshold in 2012 is marked by the vertical dashed line. The histogram bins are monthly gross earnings relative to the SGA threshold in the relevant year. The bin width is €10. The solid line beneath the empirical distribution is a sixth-degree polynomial fitted to the empirical distribution using equation (4.6). Bunching b is excess mass in the excluded range below the notch relative to the average counterfactual density in the interval $[z^L, z^*]$. Bootstrapped standard errors are shown in parentheses.

Persistency of Bunching and Dominated Behavior. Taking advantage of the longitudinal aspect of our data, we next investigate the dynamics of bunching and dominated behavior over time. More specifically, we group beneficiaries in each quarter in one of four segments as a function of their earnings z: (i) bunching segment ($z^L \leq z \leq z^*$), (ii) dominated segment ($z^* < z < z^U$), (iii) below segment ($0 < z < z^L$), and (iv) above segment ($z^U \leq z$). Figure 4.6 illustrates the fraction of beneficiaries in each segment over time for beneficiaries who in the first quarter after DI entry are in the bunching segment (left panel) or in the dominated segment (right panel).

The left panel shows that bunching is highly persistent over time. Around 60 percent of DI beneficiaries who are located in the bunching segment in the first quarter after DI entry are still bunching five years later. On the other hand, the fraction of beneficiaries in the dominated and above segment is always very low (below 10 percent). Some beneficiaries move to the dominated region, presumably because it is difficult to control earnings perfectly. Over time there is an increase in fraction of beneficiaries in the below segment, perhaps reflecting that some beneficiaries reduce their earnings due to deteriorating health.

The right panel shows that there is a drastic and fast decline in the fraction of beneficiaries in the dominated region. Five quarters after DI entry only about 30 percent of beneficiaries are still located in the dominated region and this fraction declines further to about 20 percent. The beneficiaries who remain in the dominated range in the long run are likely those with a low elasticity. Over time almost 40 percent of beneficiaries in the dominated range move to the bunching segment, suggesting that adjustment frictions prevent many beneficiaries from bunching in the short run. About 20 percent of beneficiaries move to the above segment, indicating the importance of career effects. Similarly to the left panel, there is an increase in the fraction of beneficiaries in the below segment, probably because of deteriorating work potential over time.

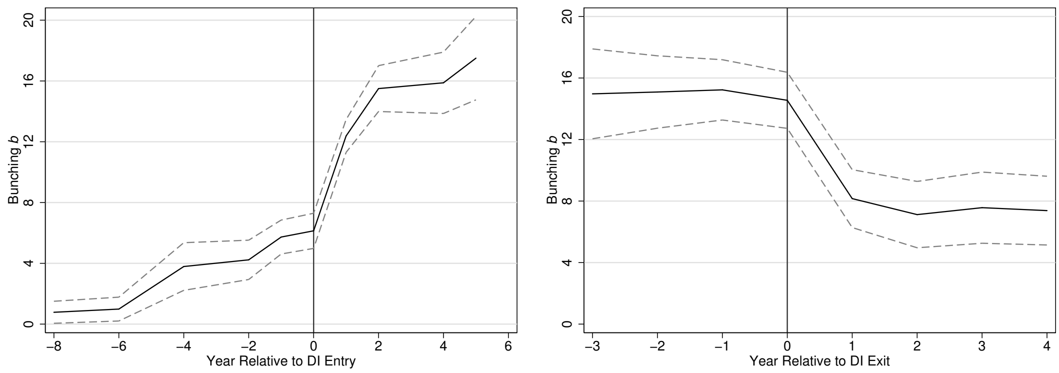
Figure 4.6: Dynamics of Dominated and Bunching Behavior over Time



Notes: The left panel shows the dynamics of dominated behavior by quarter after DI entry. The sample consists of all DI beneficiaries who are located in the dominated range $(z^*, z^U]$ in the first quarter after DI entry. The right panel shows the dynamics of bunching behavior by quarter after DI entry. The sample consists of all DI beneficiaries who are located in the bunching segment $[z^L, z^*]$ in the first quarter after DI entry. "Below" and "above" denote the earnings intervals $(0, z_L)$ and (z_U, ∞), respectively.

Speed of Earnings Adjustment. The jump in implicit tax liability at the SGA threshold is much smaller for individuals not receiving DI compared to individuals on the DI program. Individuals on DI lose a portion of their benefits and have to pay social security contributions while those not on DI only have to pay social security contributions. As a consequence, we would expect to see more bunching while individuals receive DI benefits and less bunching before individuals enter the program or after they exit the program due to medical recovery. The availability of data both before individuals enter and after they exit the DI program allows us to examine how fast bunching adjusts to changes in tax liability at the SGA threshold.

Figure 4.7: Bunching Before/After DI Entry and Before/After DI exit



Notes: The left panel shows the amount of bunching b for different years before and after DI entry (vertical solid line). The sample consists of DI recipients who are working at least once in the first five years after program entry. The right panel shows the amount of bunching before and after exit from the DI program (vertical solid line). The sample consists of DI recipients who exit the DI program between 2004 and 2007; exits into the old-age pensions are excluded. The dashed lines denote 95 percent confidence intervals.

The left panel of Figure 4.7 plots estimates of b and 95 percent confidence intervals in the years before and after DI entry for beneficiaries who work at least once in the first five years on the program. The earnings distribution around the SGA threshold in each year is shown in Figure 4.11 in Appendix B. The amount of bunching is close to zero eight to six years before program entry and increases steadily to 5.41 in the year of program entry. Excess bunching jumps to 12.74 in the first year on the program and continues to increase to 18.39 five years after program entry, highlighting the persistency of bunching over time. This pattern suggests that some individuals adjust earnings before DI entry in anticipation that they will be awarded benefits, but most of the adjustment takes place within the first year on the program.

We can perform a similar analysis when individuals exit the DI program by examining whether excess bunching becomes smaller or even disappears as individuals leave the DI program. The right panel of Figure 4.7 plots estimates of b and 95 percent confidence intervals for each year before and after DI exit for recipients who lose benefits between 2004 and 2007. Figure 4.12 in Appendix B displays the corresponding earnings distributions around the SGA threshold. There is a substantial reduction in bunching from 15.81 in the last year on the program to 8 in the first year after program exit. However, excess mass around the SGA threshold is highly persistent after program exit; the amount of bunching is still almost 8 four years after program exit. One potential explanation for the slow earnings adjustments is that many individuals who lose benefits may be speculating that they will return to program in the near future. We find that around 66 percent of DI recipients who lose benefits appeal the decision and 72 percent of those who appeal are eventually readmitted to the program.

4.4.2 Estimating Labor Supply Responses and Elasticities

Main Results. In this section we present estimates of earnings elasticities, by combining the nonparametric evidence on bunching presented above with the framework in Section 4.3. Specifically, we derive an estimate for the earnings response Δz^* using the point of convergence z^U and then solve equation (4.3) numerically for the elasticity e. The results are presented in Table 4.2, which displays the amount of bunching in column (2), the earnings response in column (3), and the elasticity in column (4). Panel A of Table 4.2 shows that the earnings response is very large and highly significant. DI beneficiaries who bunch would increase earnings by up to €400 per month or 91 percent more than the SGA threshold. Even though the estimated earnings response is large, the implied earnings elasticity is modest with 0.206. The earnings elasticity is robust over time; separate elasticity estimates for the first seven years after DI entry range from 0.132-0.201, as shown in Table 4.5 in Appendix A. Our estimates are comparable to the estimates presented in Gelber et al. (2013) who study earnings responses to the Social Security Earnings Test in the U.S. and around twice as large as the estimates presented in Kleven and Waseem (2013) who exploit notches in Pakistan's income tax schedule. Both this study and Gelber et al. (2013) focus on discontinuities at a low earnings thresholds where most individuals work only part-time. Since part-time workers can adjust working hours more easily, they tend to be more responsive to taxes than full-time workers.

It is important to keep in mind that our approach relies on excess and missing mass around the SGA threshold and therefore provides a good estimate for the work capacity of beneficiaries who are located around the SGA threshold. The earnings elasticity might differ in countries with a different SGA threshold than in Austria if the elasticity is heterogeneous across subgroups of beneficiaries and if characteristics of beneficiaries around the SGA threshold vary with its level. Given that Austria's earnings threshold is quite low, it is likely that beneficiaries around the SGA threshold have a low work potential. Consistent with this view, Table 4.1 shows that beneficiaries around the notch had lower earnings in their last job compared to the full population of beneficiaries. This suggests that our earnings elasticity may represent a lower bound.

Since not all beneficiaries have the same work capacity, the impact of the SGA threshold on earnings is likely to differ across beneficiaries. Our estimation strategy can be used to test for heterogeneity in earnings elasticities, provided that the number of observations

Table 4.2: Earnings Elasticities for Full Sample and Subgroups

	Bunching b	Earnings response Δz^*	Earnings elasticity e
A. Full sample			
	14.54***	400***	0.206***
	(0.470)	(16.41)	(0.018)
B. Age			
< 35	14.54***	690***	0.695***
	(1.877)	(224.9)	(0.270)
$35 - 49$	14.96***	400***	0.232***
	(0.720)	(27.01)	(0.031)
$50 - 56$	14.12***	380***	0.143***
	(0.618)	(17.87)	(0.018)
C. Gender			
Men	14.61***	390***	0.118***
	(0.698)	(21.57)	(0.021)
Women	14.22***	390***	0.316***
	(0.669)	(23.78)	(0.033)
D. Health impairment			
Mental	12.24***	340***	0.167***
	(0.808)	(30.00)	(0.034)
Physical	14.65***	400***	0.164***
	(1.243)	(42.55)	(0.043)
Other	15.88***	460***	0.269***
	(1.142)	(65.93)	(0.068)
E. Worker status			
Blue-Collar	11.76***	310***	0.115***
	(0.685)	(21.57)	(0.023)
White-Collar	20.32***	670***	0.361***
	(1.295)	(148.0)	(0.120)
F. Company status			
No Jobs Below SGA	14.11***	350***	0.153***
	(0.509)	(15.80)	(0.017)
Jobs Below SGA	24.31***	1050***	0.871***
	(1.913)	(40.88)	(0.040)

Notes: The table presents estimates of bunching b, the earnings response Δz (based on the point of convergence z^U between the observed and the counterfactual density), and the structural earnings elasticity e. Standard errors in parentheses are obtained using a bootstrap procedure where we sample from the population with replacement. The standard deviation of the distribution is shown in brackets. All estimates are based on a sixth-order polynomial fitted to the empirical earnings distribution. Significance levels: *** = 1%, ** = 5%, * = 10%.

within each subgroup is sufficiently large to detect excess mass and missing mass. In Panels B-E of Table 4.2 we present estimates of the impact of the SGA threshold for groups defined by age, gender, health impairment, and worker status. There is significant heterogeneity in the responsiveness to the SGA threshold. Panel B illustrates that bunching and elasticities are larger for DI beneficiaries below age 50 than for DI beneficiaries above age 50. This finding is consistent with existing evidence that younger DI beneficiaries exhibit the highest responsiveness to financial work incentives (Kostol and Mogstad, 2014). As Panel C shows, female DI recipients are more responsive to financial incentives than their male counterparts. Previous studies have found a similar pattern in other contexts (see, e.g., Chetty et al., 2011). There are also significant differences across impairment types, as illustrated in Panel D. DI recipients with mental and physical disorders are less responsive compared to DI recipients with other impairments. Finally, Panel E shows that white-collar workers are more responsive to financial incentives than blue-collar workers. The reason is that eligibility criteria for disability benefits are less strict for white-collar workers compared to blue-collar workers.[12] As a consequence, white-collar beneficiaries are healthier on average than blue-collar beneficiaries, facilitating adjustments in earnings to financial incentives.

Since firms are also required to pay social security contributions if workers earn above the SGA threshold, firms may have an incentive to help workers respond to taxes by offering jobs at the threshold. Such firm behavior would amplify the bunching response. To examine firm incentives, we group firms into two categories according to whether they employ workers below the SGA threshold who are not on the DI program. Panel F shows that bunching is substantially larger in firms who employ non-DI workers below the threshold (24.31) compared to firms who do not employ non-DI workers below the threshold (14.11). This pattern suggest that firms help coordinate beneficiaries' employment response.

Robustness Checks. Table 4.3 presents several robustness checks to examine the sensitivity of our estimates to different subsamples and specifications. First, we take into account that the point of convergence z^U may overstate optimization frictions because, as Figure 4.6 shows, not all beneficiaries in the dominated range move to the SGA threshold over time either due to low elasticities or career effects. In particular, we multiply the observed mass in each bin above the threshold by the fraction of beneficiaries who are bunching five years after DI entry and then re-estimate both the earnings response and the elasticity. This approach yields a lower earnings response of €270 and a lower elasticity

[12]White-collar workers are classified as disabled if their ability to work is reduced to less than 50 percent in the last occupation, while blue-collar workers are only eligible if they suffer a reduction in the ability to work of 50 percent or more relative to a healthy person in any reasonable occupation that the individual is able to carry out.

of 0.064. Second, we use an alternative approach to estimate the counterfactual density which incorporates intensive responses above the SGA threshold due to the increase in the marginal tax rate by dt. Specifically, we shift the bin counts to the right of z^U upward until the area under the counterfactual and the empirical distribution is identical. The elasticity estimate using the modified counterfactual density is similar (0.163). This is not surprising given that the density is almost flat above the SGA threshold, implying that the interior shift has little impact on the bin counts. As discussed in section 4.3, we derive the elasticity estimates assuming a quasi-linear utility function, which rules out income effects. To relax this assumption, we also estimate a model that does not depend on the functional form of the underlying utility and find a slightly larger elasticity of 0.247. The magnitude of the behavioral responses depends also on the shape of the counterfactual density. We examine the robustness of our results with respect to the shape of the counterfactual by varying the degree of the polynomial in equation (4.6). We find that the elasticity is not very sensitive in this respect.

Table 4.3: Earnings Elasticities, Robustness Checks

	Bunching b	Earnings response Δz^*	Earnings elasticity e
Fraction bunching after 5 years	14.54***	270***	0.064***
	(0.470)	(9.58)	(0.011)
Counterfactual with kink response	14.36***	360***	0.163***
	(0.475)	(13.20)	(0.015)
No functional form assumption	15.34***	430***	0.247***
	(0.539)	(17.73)	(0.018)
Alternative polynomials			
5th order polynomial	18.01***	430***	0.239***
	(0.473)	(13.36)	(0.015)
7th order polynomial	14.27***	540***	0.357***
	(0.516)	(35.57)	(0.038)
Workers not on DI program	3.94***	400***	0.787***
	(0.036)	(14.93)	(0.04)

Notes: The table presents estimates of bunching b, the earnings response Δz (based on the point of convergence z^U between the observed and the counterfactual density), and the structural earnings elasticity e. Standard errors in parentheses are obtained using a bootstrap procedure where we sample from the population with replacement. The standard deviation of the distribution is shown in brackets. All estimates are based on a sixth-order polynomial fitted to the empirical earnings distribution. Significance levels: *** = 1%, ** = 5%, * = 10%.

The change in the implicit tax rate at the SGA threshold is not only driven by the reduction in DI benefits, but also by the fact that individuals have to start paying social security contributions on all earnings. This rule implies that for individuals who are not receiving DI benefits the tax liability also changes discontinuously at the SGA threshold. The last row of Table 4.3 shows that among employed individuals who are not on DI bunching is about four times smaller compared to DI beneficiaries (b=3.94). Yet, the implied earnings elasticity is significantly larger (e=0.787). As Figure 4.10 in Appendix

B illustrates, the reason is that frictions are very large for this group of workers because there is very little missing mass just to the right of the SGA threshold. Since the change in tax liability at the SGA threshold is smaller for this group, the utility gain from moving to the threshold is lower as well, making it less attractive to adjust earnings if there is a fixed adjustment cost.

We complement our empirical analysis with an estimation of the counterfactual labor force participation rate of DI recipients had they not received DI benefits. This exercise sheds light on the external validity of the earnings elasticity estimates for Austria. We follow the approach by Bound (1989) who uses the labor force participation rate of rejected DI applicants as an estimate of the counterfactual labor force participation rate of DI recipients.[13] Table 4.6 in Appendix C shows that being awarded DI benefits leads to a 22.7-27 percentage point drop in employment. These estimates are very close to OLS estimates for the U.S. (Maestas et al., 2013) and for Norway (Kostol and Mogstad, 2014). Table 4.7 displays corresponding estimates for a sample of DI recipients whose benefits are terminated due to medical recovery. The estimates are quite similar to the results for DI entrants, suggesting that many DI recipients have considerable work capacity. They are in line with the estimates presented in Moore (2015) who studies the effects of the removal of drug and alcohol addictions as qualifying conditions in the U.S. Overall, the similarity of the estimates indicate that the work capacity of DI recipients in Austria is comparable to that of DI recipients in other countries.

4.4.3 Fiscal Effects and Policy Implications

This section discusses the fiscal effects of the SGA threshold for the government and the associated policy implications. More specifically, using data for the year 2012 we investigate the fiscal impacts of two hypothetical policy changes. Under the first policy, DI beneficiaries would keep full benefits if earnings exceed the SGA threshold (but they would still have to pay social security contributions). Under the second policy, DI beneficiaries would lose benefits more gradually. Specifically, they would lose €1 of benefits for every €2 of earnings above the SGA threshold. This policy is currently being tested in the U.S. and is known as the "$1 for $2 benefit offset" (Wittenburg et al. (2015)).

We are interested on the long-run impact of each policy on DI benefits paid, payroll taxes received, and government net expenditures. To calculate these effects, we proceed in several steps. First, we calculate the new tax parameters ΔT and Δt under each policy.

[13]This approach arguably yields an upper bound because rejected DI applicants are likely to be in better health on average than DI recipients. However,Autor et al. (2015) show that the Bound approach is not an upper bound if there are unobservable factors that are negatively correlated with subsequent labor supply–such as unobserved labor force attachment or application processing time.

Table 4.4: Annual Fiscal Effect of Abolishing the Notch

	Status quo (million €)	Abolish DI notch (million €)	€1 for €2 benefit offset (million €)
DI benefits	1025.5	7.1 (0.7%)	-0.8 (-0.1%)
Payroll tax revenues	15.1	1.5 (21.1%)	0.8 (5.3%)
Net expenses	1010.4	5.6 (0.6%)	1.5 (0.2%)
Induced entry elasticity		-0.09	0.10

Notes: All money amounts are in 2012 Euros.

For example, under the benefit offset policy we have $\Delta T = 0$ and $\Delta t = 0.5$. We then feed these tax parameters and our estimate of the earnings elasticity ($e = 0.206$) into equation 4.3 to obtain an estimate of the earnings response $\Delta z'$ under the new tax policy. This estimate implies that beneficiaries who stopped bunching due to the policy change would now earn in the interval $(\Delta z^* - \Delta z')$. Given knowledge of the earnings distribution of beneficiaries who stop bunching, we can calculate the change in payroll tax revenues and DI benefits induced by the policy change.[14]

The results for the two hypothetical policy changes are reported in Table 4.4. The first column shows that under the status quo the government spends €1,025.5 million on DI benefits per year and receives €15.1 million in payroll taxes. As shown in the second column, abolishing the DI notch generates additional DI benefit payments of €7.1 million, because beneficiaries who earn above the SGA threshold now receive full benefits.[15] The policy generates additional payroll tax revenues as some beneficiaries work more, but this effect is too small to offset the rise in DI benefit payments so that annual net government expenses increase by €5.6 million. Implementing a €1 for €2 benefit offset policy, on the other hand, would not only increase payroll tax revenues but also reduce DI benefit payments. The reason is that DI beneficiaries who increase earnings above the threshold lose part of their benefits, but this loss is offset by higher earnings so their total income still increases.

Our calculations above ignore the possibility that relaxing the earnings restrictions could induce more program entry by those able to earn above the SGA threshold.[16]

[14]Since DI benefits vary across beneficiaries, we calculate the policy-induced change in DI benefits using the average DI benefits of beneficiaries just below the SGA threshold.

[15]In our calculations, we ignore that the increase in DI benefits may induce beneficiaries above the SGA threshold to reduce their earnings (and payroll tax contributions) through an income effect.

[16]Making the earnings rules more generous could also lead to fewer program exits by current beneficiaries. However, this effect is likely to be small given that the DI exit rate is already very low under the current rules (around 1.6 percent per year).

Because the SGA threshold is identical for all beneficiaries and has not changed during the observation period (except for small inflation adjustments), we are not able to estimate the size of induced entry that may occur if earnings restrictions were to be relaxed. However, we can calculate how elastic DI program inflow would need to be to lead to an increase in government net expenditure. We follow Kostol and Mogstad (2014) and calculate an elasticity of induced entry, defined as the percentage increase in the number of DI beneficiaries relative to the percentage change in disposable income as a DI beneficiary. The abolishment of the DI notch yields a negative induced entry elasticity of -0.09 because this policy increases net expenses. The benefit offset policy yields a positive induced entry elasticity of 0.10. However, this estimate is well below the 1.2 elasticity that Mullen and Staubli (2015) find in the context of Austria, suggesting that after accounting for induced entry responses both policies would increase government net expenses.[17]

4.5 Conclusion

Many countries specify a substantial gainful activity (SGA) threshold in their DI program and if earnings exceed the SGA threshold for an extended period of time DI beneficiaries lose part or all of their benefits. This rule results in a discontinuous change in tax liability at the SGA threshold, creating a strong incentive for many beneficiaries to park earnings just below the SGA threshold. In this paper, we have examined whether DI recipients adjust their earnings because of the SGA threshold as well as how elastic their earnings are to changes in financial incentives.

Using a large and salient notch located at the SGA threshold in Austria's DI program, we provide transparent and credible documentation of behavioral earnings responses of DI beneficiaries. We find evidence for large and sharp bunching just below the SGA threshold and missing mass just above the SGA threshold, suggesting that many DI recipients would earn considerably more in the absence of the notch at the SGA threshold. Our estimation approach implies that the excess number of DI beneficiaries at the threshold equals the total number that should be observed with monthly earnings up to €400 higher. This effect represents a substantial 91 percent increase relative to the monthly SGA threshold of €439.

While the earnings responses to the SGA threshold are large, the elasticities driving those responses are modest, even after taking into account that observed earnings re-

[17]The two policies likely affect government expenses also through extensive labor supply responses, as the reduction in the implicit average tax rate induces some beneficiaries to enter the labor force. Such extensive responses generate additional payroll tax revenues and lead to a higher elasticity of induced entry. However, it is unlikely that the extensive responses would be big enough to get an elasticity of induced entry of 1.2 or more. Thus, even after accounting for extensive responses both policies would likely lead to an increase in government net expneses.

sponses are attenuated by adjustment frictions. We estimate that the earnings elasticity with respect to the implicit net-of-tax rate is 0.206, suggesting a relatively low responsiveness of earnings to financial incentives. The reason is that notches create extremely large implicit marginal tax rates and thus behavioral responses are large, even when elasticities are quite small. The elasticity estimates are heterogeneous across subgroups of the population, with women and younger age groups being more responsive to financial incentives compared to men and older age groups.

Our results are derived in the context of Austria and one needs to exercise caution when applying these conclusions to other countries. The DI program in Austria shares similarities with DI programs in other countries in terms of size and composition of beneficiaries. Moreover, our estimates of the counterfactual labor force participation rate of DI beneficiaries using rejected applicants as a control group are similar to those found in recent studies, suggesting that our findings may be informative for other settings as well. However, there are also some characteristics that are distinct from other programs, most notably the level of the SGA threshold. This difference is important because our estimation strategy exploits variation in earnings of beneficiaries located around the SGA threshold. The elasticity may be different in countries with a different SGA threshold than in Austria.

Our framework is useful to shed light on the fiscal effects of policy reforms encouraging work among DI beneficiaries by reducing the implicit tax on earnings. Our calculations suggest that replacing the notch at the SGA threshold with a €1 benefit offset for every €2 of earnings would increase work and reduce government expenditures. However, allowing DI recipients to earn more while keeping benefits may increase the incentive to apply for DI benefits. While we cannot estimate the level of induced entry that would occur if earnings restrictions were relaxed, we instead calculate how elastic entry responses would have to be to increase net expenditure. We find that the elasticity of program inflow to changes in benefits estimated in previous studies is above our break-even elasticity, suggesting that government net expenditures would increase after accounting for induced entry responses.

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4.6 Appendix

A Additional Tables

Table 4.5: Earnings Elasticities in Different Years After DI Entry

	Bunching b	Earnings response Δz^*	Earnings elasticity e
1 Year after Entry	13.37***	400.0***	0.178***
	(0.603)	(21.80)	(0.023)
2 Years after Entry	15.81***	400.0***	0.166***
	(0.690)	(22.81)	(0.023)
3 Years after Entry	16.81***	420.0***	0.188***
	(0.873)	(32.63)	(0.033)
4 Years after Entry	14.97***	370.0***	0.132***
	(0.916)	(27.74)	(0.027)
5 Years after Entry	15.81***	390.0***	0.153***
	(1.118)	(37.67)	(0.037)
6 Years after Entry	17.76***	430.0***	0.183***
	(1.308)	(48.95)	(0.046)
7 Years after Entry	17.64***	440.0***	0.201***
	(1.598)	(56.55)	(0.055)

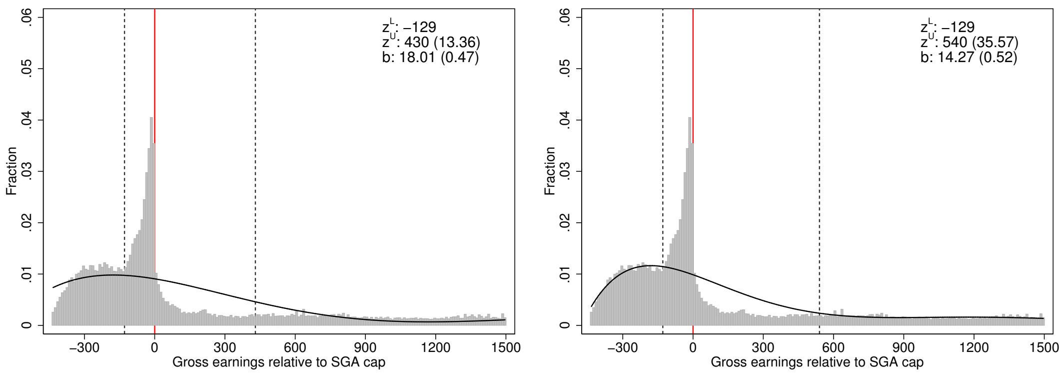
Notes: The table presents estimates of bunching b, the earnings response Δz (based on the point of convergence z^U between the observed and the counterfactual density), and the structural earnings elasticity e for different years after entry into DI. Standard errors in parentheses are obtained using a bootstrap procedure where we sample from the population with replacement. The standard deviation of the distribution is shown in brackets. All estimates are based on a sixth-order polynomial fitted to the empirical earnings distribution. Significance levels: *** = 1%, ** = 5%, * = 10%.

B Additional Figures

Figure 4.8: Distribution of Annual Earnings Around the SGA Threshold

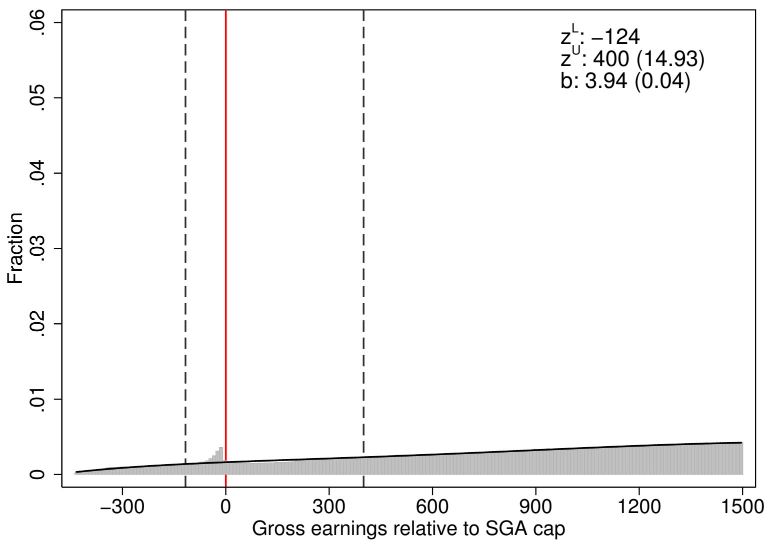
Notes: The figure shows the earnings distribution of annual gross earnings around the annual SGA threshold (marked by the vertical solid line) for DI beneficiaries between 2001 and 2012. The histogram bins are annual gross earnings relative to the SGA threshold in the relevant year. The bin width is €120.

Figure 4.9: Estimated Counterfactual Earnings Distributions Around the SGA Threshold for Fifth-degree (left panel) and Seventh-degree (right panel) Polynomials



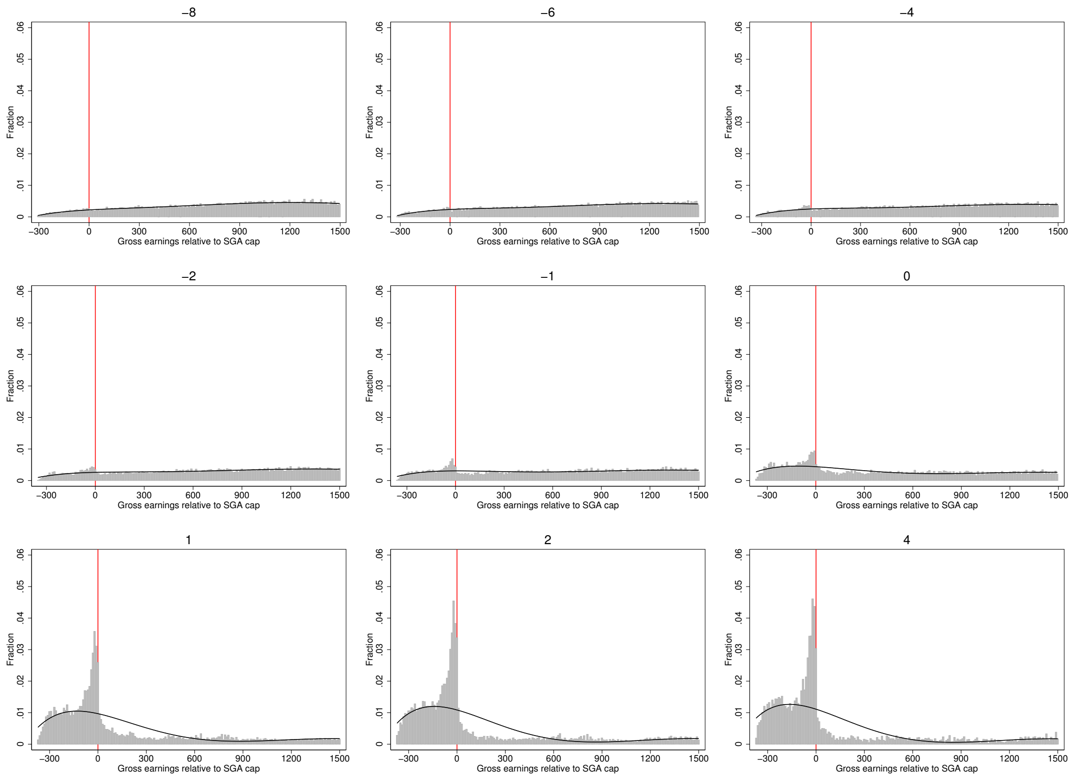
Notes: The figure shows the earnings distribution of monthly gross earnings around the SGA threshold (marked by the vertical solid line) for DI beneficiaries between 2001 and 2012. The excluded range $[z^L, z^U]$ is marked by vertical dotted lines. The histogram bins are monthly gross earnings relative to the SGA threshold in the relevant year. The bin width is €10. The solid line beneath the empirical distribution in the left (right) panel is a fifth-degree (seventh-degree) polynomial fitted to the empirical distribution using equation (4.6). Bunching b is excess mass in the excluded range below the notch relative to the average counterfactual density in the interval $[z^L, z^*]$ and z^U has been estimated such that missing mass equals bunching mass. Bootstrapped standard errors are shown in parentheses.

Figure 4.10: Earnings Distribution Around the SGA Threshold for Workers not Receiving DI Benefits



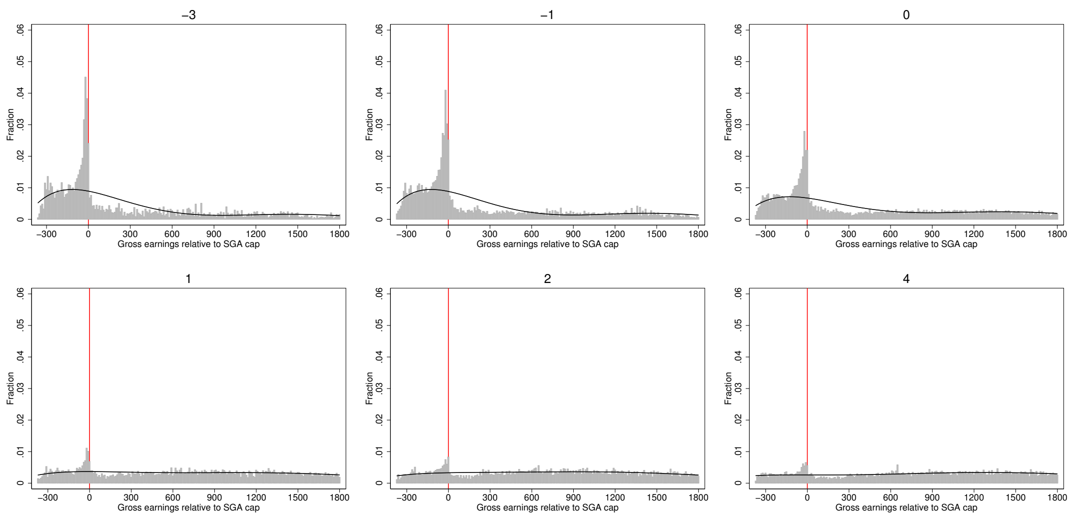
Notes: The figure shows the earnings distribution of monthly gross earnings around the SGA threshold (marked by the vertical solid line) for individuals not receiving DI benefits between 2001 and 2012. The excluded range $[z^L, z^U]$ is marked by vertical dotted lines. The histogram bins are monthly gross earnings relative to the SGA threshold in the relevant year. The bin width is €10. The solid line beneath the empirical distribution is a sixth-degree polynomial fitted to the empirical distribution using equation (4.6). Bunching b is excess mass in the excluded range below the notch relative to the average counterfactual density in the interval $[z^L, z^*]$ and z^U has been estimated such that missing mass equals bunching mass. Bootstrapped standard errors are shown in parentheses.

Figure 4.11: Earnings Distribution Around the SGA Threshold Before and After DI Entry



Notes: The figure shows the earnings distribution of monthly gross earnings around the SGA threshold (marked by the vertical solid line) for DI beneficiaries 8, 6, 4, 2, 1 years before DI entry and 0, 1, 2, 4 years after DI entry. The sample consists of DI beneficiaries who are working at least once in the first five years after program entry. The histogram bins are monthly gross earnings relative to the SGA threshold in the relevant year. The bin width is €10. The solid line beneath the empirical distribution is a sixth-degree polynomial fitted to the empirical distribution using equation (4.6).

Figure 4.12: Earnings Distribution Around the SGA Threshold Before and After DI Exit

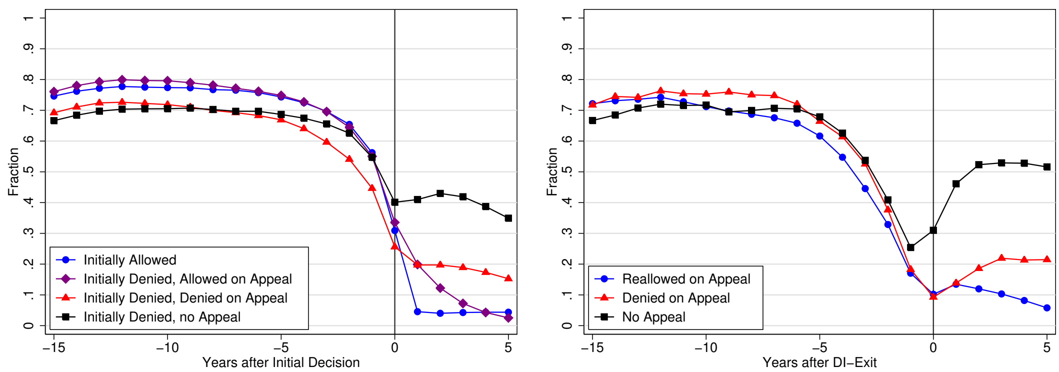


Notes: The figure shows the earnings distribution of monthly gross earnings around the SGA threshold (marked by the vertical solid line) for DI beneficiaries 3, 1 years before exit from the DI program and 0, 1, 2, 4 years after exit from the DI program. The sample consists of DI recipients who exit the DI program between 2004 and 2007; exits into the old-age pensions are excluded. The histogram bins are monthly gross earnings relative to the SGA threshold in the relevant year. The bin width is €10. The solid line beneath the empirical distribution is a sixth-degree polynomial fitted to the empirical distribution using equation (4.6).

C Labor Supply Response Using Bound-Approach

This section presents estimates of the counterfactual labor force participation rate of DI recipients had they not received benefits. We follow the method by Bound (1989) who uses the labor force participation rate of rejected DI applicants in the U.S. as an estimate of the counterfactual labor force participation rate of DI recipients. This approach arguably yields an upper bound because rejected DI applicants are likely to be in better health on average than DI recipients.[18] We extend his approach in two dimensions: first, our data contain information on the receipt of unemployment and sick leave benefits allowing us to examine benefit substitution between DI and related social insurance programs. Second, we also estimate the effects of terminating DI benefits using the labor force participation rate of beneficiaries whose program eligibility ceased due to medical recovery as an upper bound of the labor force participation rate of beneficiaries who continue on the program. This estimate is informative on the effectiveness of return-to-work policies in returning beneficiaries to the labor force.

Figure 4.13: Employment Before and After Initial Decision (left panel) and DI-Exit (right panel)



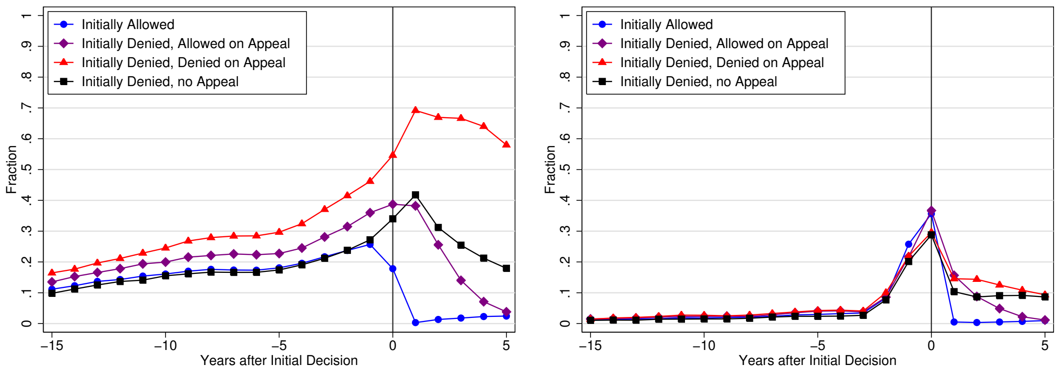
Notes: The figure shows employment rates relative to the year of the initial decision (left panel) and relative to the year of DI-exit (right panel) for different groups of individuals. Employment is measured as having positive working days in the year in consideration. The sample consists of all initial applicants in the years 2004-2007 (left panel) and all DI recipients who left the DI program in the years 2005-2007 (right panel), except for those who have been transferred to the old-age pension program.

The left panel of Figure 4.13 displays the employment rate of 2005-2007 applicants up to fifteen years before and five years after their initial determination. Employment is defined as having positive working days in a given year. Before the initial determination, the employment rate of applicants who were initially allowed is very similar to that of applicants allowed on appeal. There is a sharp drop in employment in the determination year and by three years after the decision employment rates are relatively constant at

[18]Autor et al. (2015) note that the Bound approach is not an upper bound if there is omitted variable bias from unobservable factors that are negatively correlated with subsequent labor supply–such as unobserved labor force attachment or application processing time.

around 5 percent for both groups. In contrast, ultimately denied applicants have lower employment rates before the initial determination and significantly higher employment rates after the initial determination. Denied applicants who do not appeal and those who appeal have similar employment rates prior to the initial decision, but employment rates are around twice as large for denied applicants who do not appeal after the initial decision. The right panel of Figure 4.13 shows the employment rate of DI recipients who lost their benefits between 2005 and 2007 up to fifteen years before and five years after DI exit. Before the withdrawal of benefits, the employment rate of recipients who do not appeal differs only very little from that of recipients who do (successfully or unsuccessfully) appeal. The employment rate increases sharply after the withdrawal of benefits for beneficiaries who do not appeal, while the employment rate steadily declines for beneficiaries who are re-allowed. There is also a rise in the employment rate after benefit withdrawal for recipients who are denied on appeal, although the employment rate is considerably lower compared to recipients who do not appeal.

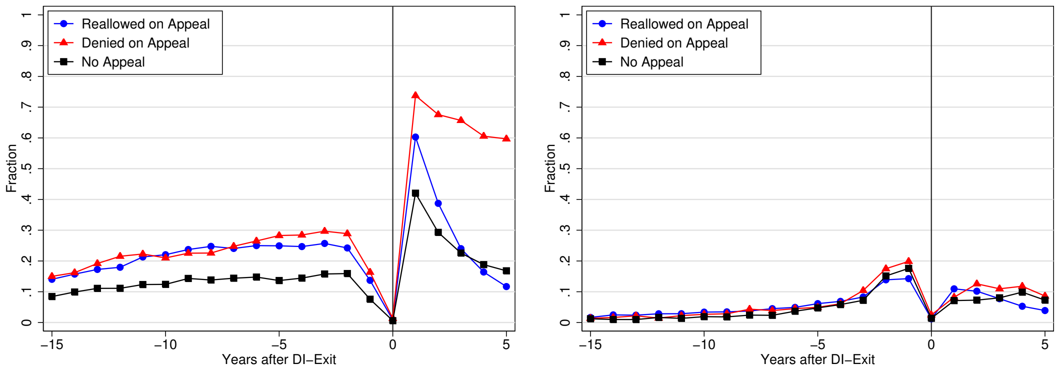
Figure 4.14: Unemployment (left panel) and Sick Leave (right panel) Before and After Initial Decision



Notes: The figure shows the share of individuals in unemployment and sick leave before and after the year of the initial decision for different groups of DI applicants. The sample consists of all initial applicants in the years 2005-2007.

In addition to disability insurance, unemployment and sick leave insurance also provide income replacement in the case of a separation from the labor market for economic or health reasons. It is likely that the receipt of disability benefits impacts unemployment and sick leave enrollment. Figure 4.14 display trends in unemployment and sick leave up to fifteen years before and five years after the initial determination, while 4.15 shows analogous trends before and after the removal of disability benefits. Both figures show that spillover effects among these government transfer programs are important. More

Figure 4.15: Unemployment (left panel) and Sick Leave (right panel) Before and After DI-Exit



Notes: The figure shows the share of individuals in unemployment and sick leave before and after the year of DI-exit for different groups of DI claimants. The sample consists of all individuals who left the DI program in the years 2004-2007, except for those who have been transferred to the old-age pension program.

specifically, in the year before the initial decision 30-50 percent of applicants are registered as unemployed and about 20 percent claim sick leave benefits. These numbers drop to zero five years after the initial determination for applicants awarded DI benefits, while the unemployment and sick leave rates remain large for ultimately denied applicants. Similarly, there is sizeable increase in the unemployment and sick leave rate in the first year after individuals lose their DI benefits. These rates decline steadily for recipients who are re-allowed to the program but remain high for those who permanently exit the DI rolls.

Table 4.6 presents OLS estimates on the impact of being awarded DI benefits on employment (positive working days), employment above SGA (e.g., earning more than €5,268 in 2012), annual earnings, registered unemployment, and sick leave.[19] The key explanatory variable *ALLOW* is equal to one if an applicant is awarded benefits (up to five years after the initial decision), and zero otherwise. Panels A-C show that receiving DI leads to a 22.7-27 percentage point drop in employment, a 19.4-22.5 percentage point drop in the probability of earning more than the annual SGA threshold, and a €4,278-€4,726 drop in annual earnings. These estimates are very close to the OLS estimates (and slightly above the IV estimates) reported in Maestas et al. (2013) for the United States. Moreover, panels D and E show that receiving DI is associated with a 35.1-39.3 percentage point decrease in unemployment and a 7.7-8.6 percentage point decrease in sick leave absence.

Table 4.7 displays corresponding estimates for the sample of DI recipients who lose benefits between 2005 and 2007. Here, *ALLOW* is equal to one if a recipient is not

[19]More specifically, the regression takes the following form $y_i = \mathbf{X_i}\beta + \gamma ALLOW_i + \varepsilon_i$, where y_i is the outcome variable of interest of applicant i, $\mathbf{X_i}$ denotes observed characteristics (past labor market experience, past average wage, and dummies for gender, occupation, region, and industry), $ALLOW_i = 1$ if the applicant is awarded DI benefits up to five years after the initial determination, and ε_i is an error term.

Table 4.6: Impact of DI Benefit Receipt on Employment, Earnings, and Transfers

Years after decision	Two	Three	Four	Five
A. Working days > 0				
Coefficient on ALLOW	-0.265***	-0.270***	-0.251***	-0.227***
	(0.003)	(0.003)	(0.003)	(0.002)
R^2	0.150	0.168	0.172	0.170
Mean dependent Variable | allowed	0.047	0.049	0.049	0.049
Mean dependent Variable | denied	0.248	0.221	0.194	0.172
B. Earnings > SGA				
Coefficient on ALLOW	-0.217***	-0.225***	-0.213***	-0.194***
	(0.003)	(0.003)	(0.002)	(0.002)
R^2	0.125	0.141	0.145	0.146
Mean dependent Variable | allowed	0.039	0.040	0.040	0.041
Mean dependent Variable | denied	0.208	0.182	0.160	0.143
C. Earnings				
Coefficient on ALLOW	-4,410***	-4,726***	-4,591***	-4,278***
	(69)	(70)	(66)	(66)
R^2	0.102	0.111	0.119	0.107
Mean dependent Variable | allowed	898	918	915	970
Mean dependent Variable | denied	4,389	3,773	3,361	3,054
D. Unemployment				
Coefficient on ALLOW	-0.381***	-0.393***	-0.384***	-0.351***
	(0.003)	(0.003)	(0.003)	(0.003)
R^2	0.226	0.252	0.261	0.244
Mean dependent Variable | allowed	0.020	0.023	0.025	0.025
Mean dependent Variable | denied	0.405	0.344	0.298	0.257
E. Sick leave				
Coefficient on ALLOW	-0.077***	-0.084***	-0.086***	-0.078***
	(0.002)	(0.002)	(0.002)	(0.002)
R^2	0.028	0.038	0.046	0.046
Mean dependent Variable | allowed	0.006	0.007	0.008	0.010
Mean dependent Variable | denied	0.105	0.086	0.071	0.062
Observations	88,562	87,285	86,114	84,997

Notes: The sample consists of first applicants for DI benefits in the years 2005-2007. Control variables include: experience past 15 years, unemployment past 15 years, sick leave past 15 years, tenure in years prior to decision, average wage, and dummies for gender, occupation, region (37) and industry (251). Significance levels: *** = 1%, ** = 5%, * = 10%.

re-allowed to the DI program, and zero otherwise. The estimates are quite similar to the results for DI entrants, suggesting that many DI recipients have considerable work capacity. More specifically, panels A-C indicate that exiting the DI programs leads to a 20.6-29.2 percentage point rise in employment, a 19.6-27.6 percentage point increase in the probability of earning above the annual SGA threshold, and a €3,975-€5,545 increase in annual earnings. These estimates are very close to the evidence presented in Moore (2015) who studies the labor supply effects of the removal of drug and alcohol addictions as qualifying conditions in the U.S. DI program. Panel D indicates that the removal of DI benefits leads to a sizeable increase in registered unemployment, while Panel E shows that by four years after program exit sick leave receipt starts to increase. Overall, the similarity of the labor supply estimates indicates that the work capacity of DI recipients in Austria is comparable to that of DI recipients in the U.S., lending support to the external validity of our analysis on the earnings response to the SGA threshold.

Table 4.7: Impact of DI Benefit Loss on Employment, Earnings and Transfers

Years after decision	Two	Three	Four	Five
A. Working days > 0				
Coefficient on ALLOW	0.206***	0.255***	0.278***	0.292***
	(0.012)	(0.012)	(0.011)	(0.011)
R^2	0.189	0.218	0.248	0.255
Mean dependent Variable | allowed	0.455	0.471	0.474	0.466
Mean dependent Variable | denied	0.187	0.159	0.141	0.118
B. Earnings > SGA				
Coefficient on ALLOW	0.196***	0.234***	0.256***	0.276***
	(0.010)	(0.010)	(0.010)	(0.010)
R^2	0.197	0.224	0.247	0.273
Mean dependent Variable | allowed	0.346	0.369	0.374	0.369
Mean dependent Variable | denied	0.099	0.083	0.066	0.044
C. Earnings				
Coefficient on ALLOW	3,975***	4,519***	5,183***	5,545***
	(215)	(220)	(221)	(221)
R^2	0.200	0.216	0.232	0.250
Mean dependent Variable | allowed	6,698	7,090	7,457	7,520
Mean dependent Variable | denied	1,653	1,510	1,214	872
D. Unemployment				
Coefficient on ALLOW	0.090***	0.184***	0.210***	0.240***
	(0.013)	(0.012)	(0.012)	(0.011)
R^2	0.101	0.100	0.089	0.119
Mean dependent Variable | allowed	0.432	0.383	0.340	0.324
Mean dependent Variable | denied	0.387	0.240	0.164	0.117
E. Sick leave				
Coefficient on ALLOW	-0.019**	-0.000	0.040***	0.029***
	(0.008)	(0.008)	(0.007)	(0.007)
R^2	0.004	0.015	0.022	0.024
Mean dependent Variable | allowed	0.092	0.091	0.106	0.078
Mean dependent Variable | denied	0.102	0.077	0.053	0.039
Observations	5,967	5,912	5,841	5,791

Notes: The sample consists of all individuals who left the DI program in the years 2005-2007, except for those who have been transferred to the old-age pension program. Control variables include: experience past 15 years, unemployment past 15 years, sick leave past 15 years, tenure in years prior to decision, average wage, and dummies for gender, occupation, region (37) and industry (251). Significance levels: *** = 1%, ** = 5%, * = 10%.

D Derivation of Equation (4.3)

This section illustrates the derivation of equation (4.3). The utility level at the SGA threshold z^* is given by

$$u(z^*) = (1-t)(s+z^*) - \frac{n^* + \Delta n^*}{1+1/e}\left(\frac{z^*}{n^* + \Delta n^*}\right)^{1+1/e},$$

where $(n^* + \Delta n^*)$ is the ability level of the DI beneficiary that is indifferent between z^* and z^I. Maximizing equation (4.2) with respect to $T(s,z) = -(1-t)s + tz + [\Delta T + \Delta tz]\mathbf{1}(z > z^*)$ implies that $z^I = (n^* + \Delta n^*)(1-t-\Delta t)^e$. Using this expression, we can write the utility at the interior point z^I as follows

$$u(z^I) = (1-t)s - \Delta T + \Delta t z^* + \frac{1}{1+e}(1-t-\Delta t)^{1+e}(n^* + \Delta n^*).$$

Setting $u(z^I) = u(z^*)$ and using the condition $(n^* + \Delta n^*) = \frac{z^* + \Delta z^*}{(1-t)^e}$, we obtain an expression that defines the elasticity e as an implicit function of :

$$(1-t)z^* + \Delta T - \Delta t z^* - \tfrac{n^* + \Delta n^*}{1+1/e}\left(\tfrac{z^*}{n^* + \Delta n^*}\right)^{1+1/e} = \tfrac{1}{1+e}(1-t-\Delta t)^{1+e}\left(\tfrac{z^* + \Delta z^*}{(1-t)^e}\right) \Leftrightarrow$$
$$\tfrac{1}{1+\Delta z^*/z^*}\left[1 + \tfrac{\Delta T/z^* - \Delta t}{1-t}\right] - \tfrac{1}{1+1/e}\left(\tfrac{1}{1+\Delta z^*/z^*}\right)^{1+1/e} - \tfrac{1}{1+e}\left(1 - \tfrac{\Delta t}{1-t}\right)^{1+e} = 0$$

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